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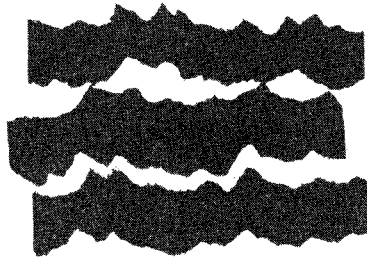
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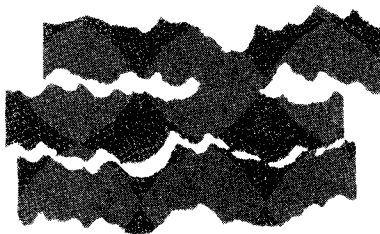


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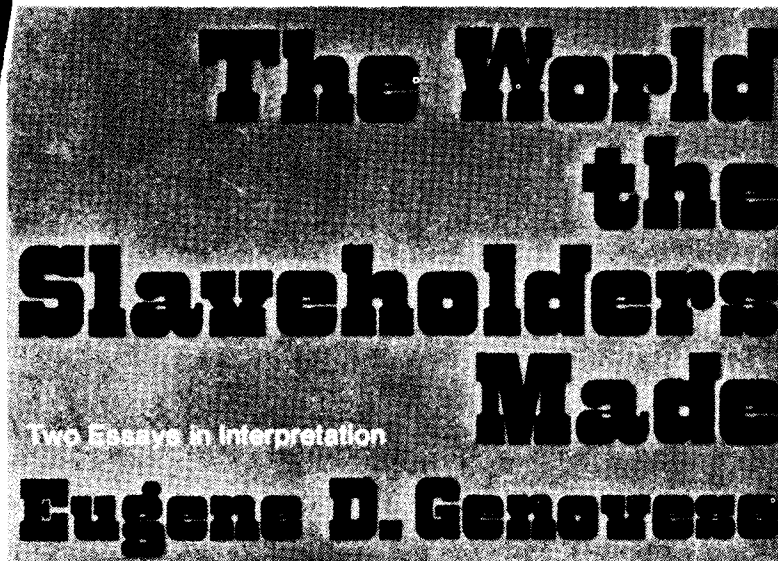
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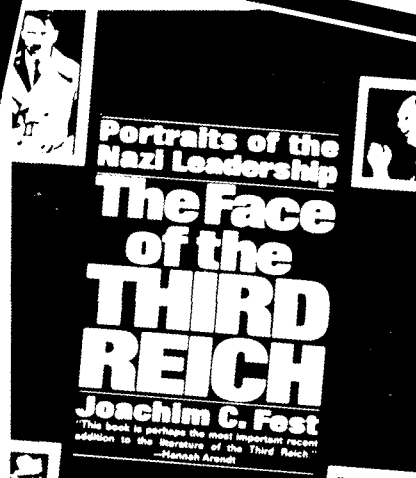
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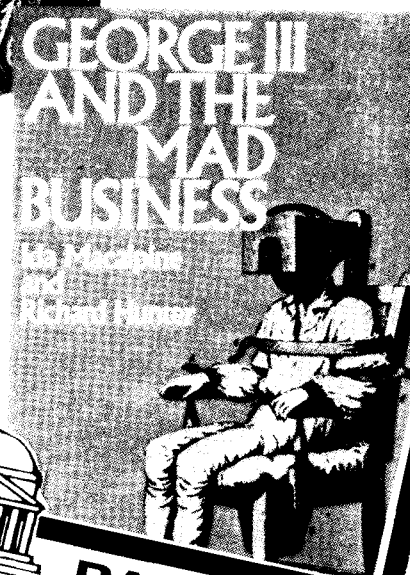
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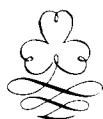
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VOLUME LXXV, NUMBER 7

DECEMBER 1970

Reviewing in the *AHR*:

An Editorial

EVERYONE talks about the publishing explosion, and a good many people are doing something about it. With greater coordination of effort, techniques of bibliographical control will in all probability, within the next ten years, simplify the lives of readers and editors and give a very different aspect to the pages of learned journals. Meanwhile, at least at the *AHR*, we are left with the traditional apparatus of reviews and lists and with an expectation of as thorough a coverage as possible of historical publishing. It is, of course, impossible to define an all-inclusive and completely predictable canon for reviewing policy; exceptions will inevitably arise from the ingenuities and sports of the publishing world and from the necessary discretion of editors. But it seems wise to make as specific a statement as possible concerning the definition and role of reviews in the *AHR* so that readers, authors, and publishers may know what to expect and what not to expect.

The number of middle-sized reviews (400–500 words) of individual books—at one time almost the sole form of reviewing—will gradually decline. There will be a somewhat greater use of brief notices, and, where possible, the number of reviews of two or more books on similar subjects will increase. Review articles will become steadily more important and will enable the reviewer—whether his subject is one or a number of books—to consider what is generally significant in the work of his field for historians of other areas. Lists of “Other Books Received” will continue to be printed, even though in time it may become possible or necessary to publish lists of articles separately.

The following rules govern the choice of books for reviewing and listing:

The *AHR* will neither review nor list novels, plays, poetry, literary criticism, and current political or social polemics or analysis. We will continue to review works of current history. In the past we have neglected books in literary and art history, and we hope to increase our coverage of them in the future. Similarly, works in the social sciences that can be of importance to historians will, in so far as possible, be listed and in some instances reviewed. The decisive factor will be the editors' judgment as to the usefulness of a book to serious historians. It would be a waste of both effort and space to list books that are of tangential interest, or that might one day serve as bits of historical raw material. Nor will a mere gesture to history in a brief introductory chapter of a book that lies essentially in another discipline qualify it for inclusion in our reviewing section.

The *AHR* will list, but not review, publications that are useful primarily as aids in teaching: textbooks, books of readings, and brief surveys. The editors recognize frankly that, particularly with the growing sophistication of some of this writing, this rule will occasionally exclude from review works of considerable scholarly merit and interest; on the other hand, such books are quite generally made available by publishers, so that interested readers may judge for themselves more easily than they can with scholarly monographs. From time to time, exceptions may be made, as, for example, when a book of documents is prefaced by a long introduction that is a monograph in its own right.

The *AHR* will list reprintings of rare or long-forgotten works if, in the editor's judgment, they have acquired value as sources—for example, the numerous recent reissues of nineteenth-century books relevant to the study of black history—but the *AHR* will neither review nor list routine reprintings of scholarly works, even though they may have been out of print for many years. New editions will be listed and citations given to the original reviews, but a new edition will be reviewed a second time only when it is so substantially altered as to be virtually a new work.

Although the editors welcome critical reviews, some books that at first sight appear significant may not be reviewed if the editors decide—usually on advice from potential reviewers—that they are so flawed in scholarship that they do not merit the use of space for review. It may of course be useful from time to time to make an example of such a book *pour encourager les autres*.

When a series of published documents or papers is launched, the first volume will be reviewed to inform the profession of the existence of the series and of its editorial methods and standards; thereafter, with rare exceptions, subsequent volumes will not be reviewed, although there remains the possibility of a retrospective review of a series on its completion. Intermediate volumes in

such series and occasional minor, miscellaneous, or highly technical documentary publications of governments or historical societies will be included in the regular lists of "Other Books Received." Where the material of a documentary collection warrants, however, a review will be published.

There will also appear in each issue a special section devoted to *Festschriften* and other collaborative volumes that, because of their miscellaneous character, cannot be satisfactorily reviewed. In each case separate contributions to the volume will be listed, thereby rescuing a good many essays from the oblivion to which they are consigned by current unsystematic practices. Where the unity or importance of such a volume warrants, however, a regular review will be published.

Increasing efforts will be made to examine and evaluate the growing output in countries in continental Europe, Asia, Africa, and Latin America. But, given the general nature of the *AHR*'s audience, and recognizing that works in other than Western European languages may be inaccessible to most readers and already known to specialists through their own journals, the *AHR* will probably resort less to discrete reviews than to occasional surveys by subject or country, as the pattern of publishing warrants and as the capacity of the *AHR* staff to cope with these areas increases.

Lists of articles will be published in February, June, and October; but lists of "Other Books Received" will appear in each issue.

The editors are aware that the virtues of such a program of reviewing will have corresponding defects, most notably, perhaps, in that certain books will not be reviewed so quickly as they might be if the practice of reviewing books singly were to remain dominant. But only in these ways can we effectively master the rapidly growing bulk of scholarly publication, economize on staff and publication costs, keep the *AHR* to a manageable size, and provide thoughtful and relevant coverage for the profession as a whole.

The Evolution of the Ottoman Seaborne Empire in the Age of the Oceanic Discoveries, 1453–1525

ANDREW C. HESS

BETWEEN the conquest of Ceuta in 1415 and the circumnavigation of the globe in 1522, Iberian states began the construction of maritime empires that would encompass the world. In the same period Ottoman sultans, entering upon a century of major expansion, created an Islamic seaborne empire. Corresponding in time but different in character, these two imperial maritime ventures came together along the northern coastline of the Indian Ocean to create a new frontier that firmly separated two different societies.

Until recently the study of joint Ottoman and Iberian naval expansion during the years when Christian Europe rose to the position of a world power on the oceans has not attracted attention. European historians, preoccupied with the identification of their own history, first unraveled the dramatic story of the oceanic voyages, the discoveries, and the European commercial and colonial empires, only stopping to consider how Muslim actions influenced the course of European history: Did the Ottoman Turks cause the oceanic explorations? Did the Portuguese discovery of the new route to India divert Asian trade from Mediterranean to Atlantic ports?¹ Once these questions were answered, the study of Islamic history became the work of small, specialized disciplines, such as Oriental studies, which occupied a position on the periphery of the Western historical profession. Finally the successful imperial expansion of Western states in Islamic territories during the late nineteenth and early twentieth centuries confirmed for most Europeans the idea that the history of Islam, let alone the deeds of Ottoman sultans, had little influence on the expansion of the West.

In the long run, however, the forces that stimulated Western imperialism led to a greater interest in Islamic history. The voyages of discovery, as revolutionary leaps in the technology of communication, reduced the distance between the world's societies and, therefore, brought Muslims and Christians together as

► *An assistant professor of history at Temple University, Mr. Hess, who specializes in Ottoman history, received his Ph.D. from Harvard in 1966, having studied with Stanford Shaw. An earlier article by Mr. Hess, "The Moriscos: An Ottoman Fifth Column in Sixteenth-Century Spain," was published in the AHR in 1968 (LXXIV, 1–25).*

¹ Recent scholarship answers both of these questions negatively. Vitorino Magalhães-Godinho, *A expansão quatrocentista Portuguesa* (Lisbon, 1944), 38–49; *Idem*, *L'économie de l'empire portugais aux XV^e et XVI^e siècles* (Paris, 1969), 577–835; and J. H. Parry, "Transport and Trade Routes," in *The Cambridge Economic History of Europe*, ed. E. E. Rich and C. H. Wilson (Cambridge, 1967), IV: *The Economy of Expanding Europe in the Sixteenth and Seventeenth Centuries*, 155–200.

never before. Closer contact supported a wider examination of the other culture not only to meet political needs but also to satisfy Western curiosity about the uniqueness of Muslim ways. During the nineteenth and twentieth centuries, the increasing interaction between Western merchants, missionaries, soldiers, and colonial administrators significantly widened the number of influential Europeans who were sensitive to the role of Islam in the history of the world. Concurrently, Orientalists developed the tools by which the distinct experience of the Muslim community could be explored. By the twentieth century the work of Oriental scholars and the broader cultural exposure of important groups within European society encouraged Western historians to expand their viewpoints to include the history of Islamic society during the age of European expansion.²

As one reflection of the desire for a more universal description of modern history, scholars returned to a crucial period in the imperial experience of the Western world to re-examine the interaction between Muslims and Christians during the age of the oceanic discoveries. Since many historians had argued that the balance of world power passed to European nations when Christians learned to sail over great distances on strong ships, Western historians, shifting their attention to non-Western peoples, sought to describe how the new maritime dimension of world competition in the sixteenth century affected the societies of the Near East. With these considerations in mind, the naval history of the powerful Ottoman Empire should have occupied a major position in the modern interpretations of sixteenth-century maritime history. Yet, with few exceptions, the histories dealing with the impact of Western naval power on the Muslim world contain a limited amount of information on the actions of a sixteenth-century state capable of defending itself against the West.

Now that both Western and Turkish scholars have made Islamic sources available, the internal history of the Muslim world, and especially of the Ottoman Empire, can be combined with the earlier work of Western historians to broaden the description of fifteenth- and sixteenth-century maritime history. At the same time Western interpretations of Muslim reactions to the voyages of discovery can be subjected to a new comparative analysis based on hitherto inaccessible Ottoman materials.

Since the descriptions of Western historians have set the framework for modern naval history, the explanations of Professors Arnold J. Toynbee, Carlo M. Cipolla, and William H. McNeill are selected here as representative of a point of view shared by many writers who concentrate upon the importance of the oceanic discoveries for both the history of the Near East and of the world. In his annex on the "lost opportunities" of Ottomans and Scandinavians, Professor Toynbee argues that during the first half of the sixteenth century Ottoman sea power failed to mount successful naval attacks on Portuguese fleets, either in the western Mediterranean or in the Indian Ocean, attacks that if victorious would have severed

² Bernard Lewis, "Islam," in *Orientalism and History*, ed. Denis Sinor (Cambridge, 1954), 16-33.

sea communications between Portugal and India.³ Such Ottoman naval shortcomings then led, in broad strategic terms, to the enclosure of the Muslim world by the navies of Atlantic nations, which, later on, translated their sea power into a political domination of Muslim lands.⁴ The technological and economic side of Western naval expansion is the subject of Professor Cipolla's work. In his study he claims that the balance of world power tilted in favor of the West from the end of the fifteenth century, when Atlantic sailors exploited technical innovations in the arts of sailing and cannon-making to establish superiority on the oceans. Asian societies then failed to respond to the European challenge because powerful socio-cultural factors prohibited the adoption of superior Western technology.⁵ Professor McNeill, in the most sophisticated analysis of the Muslim world on the eve of a new era, also sees 1500 as a pivotal date in Muslim as well as Western history. Limited by the Christian reconquest of Spain and the Portuguese domination of the Indian Ocean and by the rising power of Russia and other European states, and weakened by a widening internal conflict between the heterodox Safavid dynasty of Persia and the orthodox Ottoman dynasty of Europe and Asia Minor, the encircled Muslim community turned in upon itself in the sixteenth century, aged, and fell from its old central position to a subordinate rank behind the growing power of the European seagoing states.⁶

Although these brief summaries cannot do justice to the wider arguments of the above authors, they do serve to lay emphasis upon the naval failures of the Muslim world at a critical juncture in the history of the early sixteenth century. But a reading of Muslim sources for that same period produces no such negative picture of an era in Islamic history that was, for the Ottomans, filled with land and sea victories. The contrast in viewpoints suggests that both Ottoman court chroniclers and Western historians have worked to confirm the superiority of their respective cultures.

As Ottoman sources are developed and compared with descriptions of modern naval history, the need for a new view of the Islamic world's reaction to the voyages of discovery emerges. First, the fact that an Ottoman seaborne community came into being during the fifteenth and sixteenth centuries is unknown to most authors. Second, the absence of a developed body of historical literature about the Islamic world and the lack of information on social, economic, and intellectual aspects of Muslim history has conditioned most writers to approach the history of the Near East with questions related only to European matters, for which there are records. Thus histories of Western commercial activity in the Near East, of the rising political power of European commercial classes in matters concerning the Muslim world, of the Christian missionary work in Asia, of Western views of

³ Arnold J. Toynbee, "The Lost Opportunities of the Scandinavians and the 'Osmanlis,'" in *A Study of History* (London, 1934-61), II, annex VII, 444-45.

⁴ *Idem*, *Civilization on Trial* (New York, 1948), 62-96; *Idem*, *The World and the West* (New York, 1953), 18-33.

⁵ Carlo M. Cipolla, *Guns, Sails, and Empires* (New York, 1965), 126-48.

⁶ William H. McNeill, *The Rise of the West* (Chicago, 1963), 611-33, 726-30, 762-75.

Asian cultures, all have to do with only one side of a multi-cultured contact. Any accurate judgment on the Muslim response to the new Western naval expansion, however, must take into consideration such internal structures of the Islamic world as its economy, political organization, social structure, technology, and religious institutions, all of which affected the manner in which the Ottoman sultans responded to the appearance of Portuguese ships in the Indian Ocean. Finally, the sixteenth-century collision of the Ottoman and Portuguese empires in the Indian Ocean brought together states with different imperial aims. To assess, therefore, the success or failure of either Ottoman or Portuguese naval imperialism solely on the basis of Western objectives distorts the internal history of the Near East, inflates the impact of Western technology in the sixteenth century, and obscures the reasons why the age of Vasco da Gama produced a new frontier in the Indian Ocean rather than the defeat of either empire.

To mesh Turkish and European naval history is not an easy task, for there is a commonly held notion that the Turks did not engage willingly in maritime activity. As their most ancient records indicate, the Turks developed little naval experience in their limited contacts with the inland seas of Eurasia.⁷ Centuries of nomadic warfare, furthermore, ingrained in the steppe nomads notions associating aristocracy with the use of the horse and camel. Turkish warriors, unprepared by their past for either imperial or commercial seafaring, consequently tended to regard the sea as an alien medium for warfare.⁸ Paul Rycha, writing in the seventeenth century when British sea power entered the Mediterranean, summed up this landsman's attitude toward the sea by citing the statement of an Ottoman who, when questioned about the power of the Muslims on the sea, alleged that "... God hath given the sea to the Christians and the land to them [the Muslims]."⁹

Whatever the attitudes of the Turks may have been toward the sea, their invasion of Anatolia at the end of the eleventh century brought them into an area of the world that had a long maritime experience. Turkish warriors reaching the coasts of Asia Minor after the battle of Manzikert in 1071 captured a shoreline that not only had produced sailors and ships since ancient times, but also limited the ability of the Turkish mounted archers to wage war. Only by taking to the sea could the Central Asian bowmen extend their search for new lands and booty, or the Holy War, to the northern Christian states. But the Turks had more than just a problem of crossing the seas that surrounded Asia Minor, for the Byzantine Empire and the Italian republics, armed by a long history of naval warfare, opposed the northern expansion of Islam.

In the eleventh century, however, the level of Eastern Christian naval power

⁷ Henry and Renée Kahane and Andreas Tietze, *The Lingua Franca in the Levant* (Urbana, 1958), 3-10.

⁸ For Mamluk naval attitudes see David Ayalon, "Bahriyya, II, The navy of the Mamluks," in *Encyclopaedia of Islam* (hereafter *EI*²) (2d ed., Leiden, 1954-), I, 945-47; and *Idem*, *Gunpowder and Fire Arms in the Mamluk Kingdom* (London, 1956).

⁹ Paul Rycha, *Present State of the Ottoman Empire* (London, 1668), 215-16.

had declined from the decades when Byzantine admirals protected commercial routes in the northern half of the Mediterranean against Muslim attacks.¹⁰ Moreover, the antimilitary policies of the early eleventh-century governments had reduced the Byzantine fleet into a polyglot, ill-financed supporting arm of the emperor's land forces. At the same time the sea trade of the Empire passed more and more into the hands of merchants from the Italian city-states.¹¹

The appearance of Turks on Aegean shores in the last quarter of the eleventh century revealed clearly both the weakness of the Byzantine navy and the disaffection of Anatolian maritime communities toward their old masters. From the year 1080 Turkish frontier warriors occupied large sections of the Anatolian coastline, and, with the help of local shipwrights, began to construct war fleets. Although the Orthodox emperor, Alexis Comnenus, destroyed the ships of the first Turkish frontier captain, Abu'l-Qasim,¹² the Turks soon mounted a more serious naval challenge. At Smyrna, an old center of Byzantine naval activity, a Turkish warrior named Chaka, aided by Anatolian artisans, built a large fleet, which ravaged Aegean islands and threatened Constantinople. Provoked to action by the Turkish naval threat, Alexis dispatched in 1091 the largest Byzantine fleet of the eleventh century against a Turkish admiral who had been educated in the capital of the Empire and who had received assistance from the seagoing population of Asia Minor.¹³ By the turn of the century, the Byzantine rulers had gained victories over both land and sea expeditions of the Turks and subsequently managed to keep Muslims away from the coastlines of Asia Minor throughout the twelfth century. Ironically their success only accentuated naval decline in the aging Eastern Roman Empire. Dwindling resources in all areas compelled Byzantine rulers to set aside expensive naval operations. Since the seagoing abilities of Italians could be used against both Christian and Muslim opponents for the price of trade concessions that would not directly reduce the land revenues of the Byzantine ruling class, in 1082 the emperor signed trade agreements that handed over naval supremacy in the eastern Mediterranean to the Venetians.¹⁴ Following this peaceful seaborne penetration of the Eastern Roman Empire, other Italian maritime states entered the Levant as the suppliers of Christian Crusaders. Strengthened on land and sea by the Catholic West, Byzantium of the twelfth century then barricaded herself behind a series of forts that confined the Turks to central Anatolia.¹⁵ Byzantine administrators, now secure within borders defended by

¹⁰ On the history of the Byzantine navy see Archibald R. Lewis, *Naval Power and Trade in the Mediterranean A.D. 500-1100* (Princeton, 1951); Hélène Ahrweiler, *Byzance et la mer* (Paris, 1966); and Ekkehard Eickhoff, *Seekriege und Seepolitik Zwischen Islam und Abendland* (Berlin, 1966).

¹¹ For eleventh-century Byzantine decline see George Ostrogorsky, *History of the Byzantine State*, trans. Joan Hussey (London, 1956), 280-310. The Venetian reaction is discussed in Heinrich Kretschmayr, *Geschichte von Venedig* (Gotha, 1905-34), I, 177-81.

¹² Anna Comnena, *The Alexiad*, trans. Elizabeth A. S. Dawes (New York, 1967), 152-58.

¹³ During his youth Chaka was held as a prisoner at the Byzantine court. *Ibid.*, 182-220, 280; and Ahrweiler, *Byzance et la mer*, 184-86.

¹⁴ Freddy Thiriet, *La Romanie vénitienne au Moyen Age* (Paris, 1959), 36-42.

¹⁵ Ahrweiler, *Byzance et la mer*, 187. The impact of the First Crusade on the Turks in Anatolia

Italian sea power and Anatolian castles, further reduced the size of their fleet, and thereby confirmed Italian control over the sea trade of the Levant.¹⁶

The oscillations of Byzantine decline in the thirteenth century served only to entrench the Italian position along the coastline of the Empire. Following the capture of Constantinople in 1204 during the Fourth Crusade, Venetians quickly established an eastern colonial empire consisting of ports, islands, and consulates spotted along the grand trading routes of the Levant. From Venice south toward Egypt and Syria and northwest to the Black Sea, the Venetian Republic strengthened its sea lines of communication by acquiring strategic outposts such as Modon and Koron, Crete, and Negroponte. Genoa, too, gained a commercial foothold on the edge of Byzantine territories when in 1261 a Greco-Genoese alliance re-established Byzantine authority in Constantinople. By the fourteenth century sea bases and trading posts running from Chios past Constantinople into the Black Sea formed the outline of an eastern Genoese seaborne empire.¹⁷

Molded by the conditions of the Renaissance, Italian imperialism in the East had a character that set it off from the imperial rule of large Mediterranean empires. The smallness of their populations, the concern of ruling classes for commercial development, the dependence of trade on sea transportation, and the wide-ranging nature of business transactions all affected the means by which fiercely independent Italian city-states expanded. Rather than invading and administering large territories, they established limited commercial empires by placing fortified trading posts in enclaves and on islands at strategic positions on the sea routes to the Levant. Defense of these outlying regions then rested on the political ability of Italian rulers, on the economic power of the merchant republics, and on the technical superiority of Italian seamen.¹⁸

While Italians divided the land and sea domains of the Byzantine Empire between the twelfth and fourteenth centuries, the Turks of Anatolia struggled to pierce the boundaries that had confined them to the interior of Asia Minor since the time of the First Crusade in 1097. During the early portions of the thirteenth century sections of the Anatolian coast at Sinope, Antalya, and Alanya came under the authority of the sultan at Konya.¹⁹ But no sooner had the Turks of Anatolia

is covered in Claude Cahen, *Pre-Ottoman Turkey*, trans. J. Jones-Williams (New York, 1968), 84-119.

¹⁶ Thiriet, *Romanie vénitienne*, 44-49, 183-87.

¹⁷ On the importance of Chios and its relation to the Oriental trade of Genoa see Jacques Heers, *Gènes au XV^e siècle* (Paris, 1961), 363-406; and Philip P. Argenti, *The Occupation of Chios by the Genoese and their Administration of the Island 1346-1566* (London, 1958), I, 31-105.

¹⁸ Thiriet, throughout *Romanie vénitienne*, develops the outlines of Venetian imperialism. An example of Venetian tactics in the international area is contained in the summary of instruction given to the Republic's admiral in the year 1424. See also *Idem*, *Régestres des délibérations du Sénat de Venise concernant la Romanie* (hereafter *Régestres*) (Paris, 1958-61), II, 215.

¹⁹ The history of the Turkish naval frontier in the Seljuk period is treated in Herbert W. Duda, *Die Seltschukengeschichte des Ibn Bibi* (Copenhagen, 1959), 44-46, 61-68, 104-09; Ismail H. Uzunçarşılı, *Anadolu Türk Tarihi Vesikalarından: Kitâbeler* (Inscriptions from the Historical Documents of Turkish Anatolia) (Istanbul, 1927-29), II, 234-36; Osman Turan, *Türkiye Selçukluları* (The Seljuks of Turkey) (Ankara, 1958), 123-29; and Ahrweiler, *Byzance et la mer*, 307. Concerning the important question of Byzantine and Muslim institutional influences on the Seljuk naval organiza-

started to organize their naval frontiers than the Mongols defeated Turkish forces at Köse Dag in 1243, and a new situation was created in Anatolia.²⁰

The political debris of the Mongol invasion produced a patchwork quilt of Turkish military communities located along the Christian-Muslim frontier in Anatolia. Since the Byzantine Empire could no longer assemble great forces, and the Italians pursued a policy of limited coastal occupation, portions of the Anatolian shoreline quickly passed into the hands of Turko-Muslim border warriors, *ghāzīs*,²¹ who established and ruled these frontier principalities. Where the frontiersmen maintained control over regions that had seagoing traditions, they soon engaged in corsair actions. Characteristically, from southwest Anatolia, an area rich in naval history, came the renowned Muslim privateer, Umur Bey,²² who with the aid of the population around Smyrna raided throughout the Aegean in the disordered naval wars of the early fourteenth century. None of the frontier sea principalities, however, went beyond the raids and counter-raids so common to the guerrilla warfare along the entire Christian-Muslim frontier. What the corsair emirates did accomplish was to give Turks and Muslims further experience in the Holy War on the seas, an ability the Ottomans would organize and exploit.

In the course of the fourteenth century the Ottoman emirate, whose early history involved almost no exposure to the sea, began to expand. Inexorably the Ottoman sultans found themselves drawn into warfare on the sea. Balkan conquests directed from Anatolia regularly exposed Ottoman troops to attacks from the Christian galleys that dominated the Straits separating the two portions of the Ottoman state. Control of the commercial and military traffic moving to and from the Black and Aegean Seas eluded a state without a navy. Protection of Ottoman coastlines in the Balkans and Asia Minor required a fleet. Finally, no Muslim ruler could hope to command newly conquered territories, half of which were in Asia and half in Christian Europe, without removing Christian control from Constantinople. But to take the Byzantine capital meant challenging the Italian maritime states that now supplied and defended that magnificently walled city.²³

By the middle of the fifteenth century the Ottomans could no longer put off

tion, see Fuad Köprülü, *Alcune osservazioni intorno all'influenza delle istituzioni bizantine sulle istituzioni ottomane*, trans. Istituto per l'Oriente di Roma (Rome, 1953), 50.

²⁰ Cahen, *Pre-Ottoman Turkey*, 119–38.

²¹ The title given to those who dedicated themselves to war against the infidel on the frontiers. Irène Mélikoff, "Ghāzī," *EI*², II, 1043–45.

²² On the operations of this fourteenth-century Turkish corsair see Paul Lemerle, *L'Emirate d'Aydın Byzance et l'Occident* (Paris, 1957). For Muslim filibustering in the Black Sea consult Claude Cahen, "Ghāzī Çelebi," *EI*², II, 1045. The Menteşe emirate has been studied in detail by Paul Wittek, *Das Fürstentum Menteşe: Studie zur Geschichte Westkleinasiens im 13.–15. Jh.* (Istanbul, 1934).

²³ 'Āşīkpāšāzāde's chronicle provides evidence of the expansion of Ottoman naval activity. *Die altomanische Chronik des 'Āşīkpāšāzāde* (hereafter 'Āşīkpāšāzāde), ed. Friedrich Giese (Leipzig, 1929), 22, 45, 51, 60, 121, 131. From the Christian side, the deliberations of the Venetian Senate record the Italian reaction to the growth of a Turkish navy. See Thiriet, *Régestres, passim*. For an early fifteenth-century description of Gallipoli, the main Ottoman naval base, see Ruy Gonzalez de Clavijo, *Narrative of the Embassy of Ruy Gonzalez de Clavijo to the Court of Timour. . . . A.D. 1403–06*, trans. C. R. Markham, in Hakluyt Society Publications, old ser. (London, 1859), 27. The Ottoman fleet is recorded here as consisting of forty galleys.

seizing the metropolis that controlled the Bosphorus and dominated the land routes leading north into the Balkan peninsula. While Crusader columns from Europe marched toward the sultan's European territories in 1444, Christian galleys cruising the Straits between the Aegean and Black Seas, almost confined the Ottoman sultan and his army to Asia Minor. Even though Murad II subsequently defeated the European Crusaders at Varna during the winter of 1444, the Christians had now elevated the importance of the Straits, to which the Byzantine capital was the key, to a level that demanded Ottoman action.²⁴ Seven years later Mehmet the Conqueror collected his forces to begin the siege of a city whose geographical location and imperial traditions would lead to the foundation of an Ottoman seaborne empire.

While Mehmet the Conqueror prepared to take Constantinople, caravels under the direction of Prince Henry the Navigator continued the development of a Portuguese maritime empire whose lines of communication would soon envelop Africa. Controlling the Strait of Gibraltar from the Moroccan city of Ceuta since 1415, the Portuguese by the middle of the fifteenth century had occupied islands and set up forts on the western periphery of the Islamic world in a line stretching from Cape Verde to Portugal. On land, however, the transfer of the Muslim-Christian frontier from Iberia to North Africa led only to prolonged warfare, which stimulated Portuguese kings to search for a more profitable form of expansion. Factors in addition to the ceaseless hostility between the two religions²⁵ also prohibited the emergence of a new Christian-Muslim state in North Africa. The commitment of the Avis dynasty to commercial ventures,²⁶ the comparatively limited population resources of Portugal,²⁷ and the superior naval and military

²⁴ The strategic importance of the Straits is cited as the main reason for the construction of Rumeli Hisarı, the European fort on the Bosphorus, and for the conquest of Constantinople. 'Aşîkpâşâzâde, 121, 131; Ibn Kemal, *Tevârih-i Âl-i Osman VII. Defter* (The History of the Ottoman Dynasty: the Seventh Volume), photographic reproduction in vol. I, transcription and criticism in Turkish in vol. II by Şerafettin Turan (hereafter all references will be to vol. II, cited as Ibn Kemal) (Ankara, 1954-57), II, 33; and Kritovoulos, *History of Mehmed the Conqueror* (hereafter Kritovoulos), trans. Charles Riggs (Princeton, 1954), 15-16. Other original sources are collected in Halil İnalcık, *Fatih Devri üzerinde Tetkikler ve Vesikalar* (Investigations and Documents on the Reign of Mehmet the Conqueror) (Ankara, 1954), I, 1-53, 72-73, 111-21; and Selâhattin Tansel, *Osmanlı Kaynaklarına göre Fatih Sultan Mehmed'in Siyasi ve Askeri Faaliyeti* (The Political and Military Activity of Sultan Mehmet the Conqueror According to Ottoman Sources) (Ankara, 1953), 26-27, 39, 45.

²⁵ Generally, scholars have examined the role of religion in Portuguese imperial history to determine whether or not the Christian crusading spirit caused the discoveries. How Christianity and Islam of the fifteenth century interacted to influence imperial actions in the overseas territories has not been studied. Portuguese sources for the conquest period, written largely for an audience in Portugal, are full of anti-Islamic sentiments. See, for example, Gomes E. de Azurara, *The Chronicle of the Discovery and Conquest of Guinea*, trans. Charles R. Beazley and Edgar Prestage, Hakluyt Society Publications, old ser. (London, 1896-99), I, 10-17, 27-30; II, 280; and Duarte Pacheco Pereira, *Esmeraldo de Situ Orbis*, trans. George H. T. Kimble, Hakluyt Society Publications, new ser. (London, 1937), 2, 57, 62, 167. The Muslim response to the Portuguese arrival in North Africa was the creation of a new state, the Saadi, which based its claim to rule on its prosecution of the Holy War. Muḥammed al-Ufrani, *Nozhet el-Hâdi (Histoire de la Dynastie Saadienne au Maroc 1511-1670)*, trans. O. V. Houdas (Paris, 1889), *passim*; Hezârfen Hüseyin b. Ca'fer, "Tenkîh-i tevârih-i mülûk" (World History), located in the Topkapı Sarayı Müzesi Library, Istanbul (hereafter TKS), R. 1180, fol. 49^a.

²⁶ Magalhães-Godinho argues that the Avis dynasty tamed the warrior aristocracy of Portugal and directed the energy of the ancient military class into the establishment of a commercial empire. *Expansão quatrocentista Portuguesa*, 41-49, 78-79, 138-40.

²⁷ Jaime Cortesão, *O império Português no Oriente* (Lisbon, 1968), 31-48.

technology of Christian forces slowly encouraged Portuguese kings to create a commercial maritime empire that did not include the vast number of peoples, cities, and territories of northwest Africa. Thus the imperial adventure of the Portuguese skipped along the coast of West Africa, touching the shore here and there at strategic outposts, which were defended later on by many of the techniques the Venetians practiced: the division of political opponents, the employment of economic resources for political ends, and the use of superior military technology when other measures failed.²⁸

In the same year that Gomes Eanes de Zurara finished his history of the discovery and conquest of Guinea,²⁹ Mehmet the Conqueror began the siege of Constantinople. The enwalled Christians found that this sultan was able to cut the last line of support for the capital city. To the north the cannons of a newly constructed castle joined with the guns of a companion fort on the other side of the Bosphorus to choke off aid to the capital city from the Black Sea. Then, more ominously for the Christians, the sultan augmented his land siege of Constantinople by stationing a large fleet off the entrance to the Golden Horn.³⁰ Kritovoulos, the Greek historian of the Conqueror's reign, recorded the astonishment of the Byzantine leaders at this new development in the long wars with the Turks.³¹ Not since the eleventh century had Turkish galleys appeared in such strength before Constantinople; and this time the Turks were not overextended.

The Ottomans broke through the massive walls of Constantinople in the spring of 1453, and vigorously took up the Byzantine inheritance, which included, among other things, a long naval history. Whatever may have been the influence of that past on the Conqueror, the Turkish possession of the sea route between the Aegean and the Black Seas vividly demonstrated to the Italians that the land power of the Ottomans had taken on a major naval dimension.³² On the other hand, the uneven performance of their fleet during the siege demonstrated to the Ottomans the maritime requirements of an empire ruled from Istanbul.³³

²⁸ For a general view of fifteenth-century Portuguese expansion and its impact on Muslim North Africa, see Bailey W. Diffie, *Prelude to Empire* (Lincoln, 1960); C. R. Boxer, *Four Centuries of Portuguese Expansion, 1415-1825: A Succinct Survey* (Johannesburg, 1961); and Henri Terrasse, *Histoire du Maroc* (Casablanca, 1949-50), II, 67-157.

²⁹ Azurara, *Chronicle of the Discovery*, II, 293.

³⁰ Since original sources in many languages give both a variety of technical descriptions and size estimates, the numbers of ships are used here only to indicate changes in Turkish naval policy. Jehan de Wavrin describes the Ottoman fleet as consisting of eighteen galleys, sixty or seventy galliots, and sixteen to twenty small craft. *Chroniques et anciennes chroniques et anciennes istories de la Grant Bretagne, a present nomme Engleterre*, ed. William Hardy and Edward L. C. P. Hardy, in *Chronicles and Memorials of Great Britain and Ireland during the Middle Ages* (London, 1891), XXXIX, pt. 5, 252. 'Āşīkpāšāzāde, who is followed by Ibn Kemal, reports a figure of four hundred ships (p. 132). Kritovoulos records a figure of 350 ships plus transport craft (p. 37). Whatever the exact size of the fleet, the Ottoman armada represented a major change in the naval situation along the Straits whether measured in size (Clavijo, *Narrative*, 27), or in the appearance of a Muslim fleet in the Bosphorus where there had been none (Wavrin, *Chroniques*, 19-48, 65-66). Italian and Greek estimates on the number of Ottoman ships are recorded in Steven Runciman, *The Fall of Constantinople, 1453* (Cambridge, 1965), 215, 76 n.1.

³¹ Kritovoulos, 38.

³² Thiriet, *Régestres*, III, 205-06, 233-34, 239; Argenti, *Occupation of Chios*, I, 201-13.

³³ Ibn Kemal describes Ottoman naval difficulties during the siege of Constantinople (pp. 56-58).

Before settling on the offensive and defensive needs of a new naval position, the Conqueror, in an act of major importance for the Ottoman navy, organized the Orthodox Christian community within the symbiotic religious structure that characterized previous Islamic empires. The appointment of the Patriarch Genadius in 1454³⁴ to head the Greek community and the subsequent acceptance by most Christians in Ottoman territories of the *millet*³⁵ system, left little doubt that the Turko-Muslim Empire would include the majority of lands and peoples in the conquered areas of the Balkans and Asia Minor. Since the coastal populations of the Aegean and Black Seas, still largely Christian, possessed technical knowledge concerning the sea, the settlement of their denominational relation, albeit in an inferior position, under an Ottoman veneer made their maritime experience available to the sultan.³⁶ On the other hand the Conqueror's political solution for non-Muslim communities, which was part of a much wider compromise with old institutions, fixed in the Empire a multiple social structure that separated the merchant community from the upper levels of the Muslim ruling class. Similarly the Ottoman military establishment, deriving the bulk of its income from the administration of agricultural lands, remained only indirectly involved in the affairs of maritime commerce.³⁷

As soon as Mehmet took control of his new capital, he promptly showed that Istanbul was to be no ash heap of Byzantine and Latin naval structures. Maintaining the fleet's old facilities at Gallipoli, the sultan now collected carpenters, merchants, and sailors from the coastal regions of the Empire for the naval service of Istanbul. Upon acquiring a maritime technology that was largely Italian, Mehmet ordered into production naval arsenals to supply galleys for a war fleet. At the same time the Ottoman ruler encouraged the settlement in Istanbul of Christian merchants who had engaged in sea trade. Aware that the growth of the city was related to its economic prosperity, Mehmet also granted tax exemptions to the trades, repopulated the city with artisans and laborers, and launched a public works program.³⁸

Stunned by the fall of Constantinople, Christian leaders speculated on what

³⁴ Āshīkpāšāzāde notes the Christian threat on the seas (p. 143). Kritovoulos, whose self-interest was involved, records the Ottoman need for a strong fleet (pp. 141-42).

³⁵ Franz C. H. Babinger, *Mahomet II Le Conquérant et son temps 1432-1481*, trans. H. E. Del Medico (Paris, 1954), 130-31.

³⁶ In the fifteenth and sixteenth century this term meant "religious community." The historical roots of this institution and its function in the Ottoman Empire are given in H. A. R. Gibb and Harold Bowen, *Islamic Society and the West* (London, 1950-57), I, pt. 2, pp. 207-61.

³⁷ Fifteenth- and sixteenth-century Ottoman population surveys for the port of Gallipoli document the slow conversion of the Greek maritime population to Islam. Halil Inalcik, "Gelibolu," *ET*², II, 983-87. For the upper-class association with the Ottomans, see Kritovoulos, 11-12.

³⁸ On the Ottoman military-administrative ruling class consult Halil Inalcik, "Ghulām, iv-Ottoman Empire," *ET*², II, 1085-91; and Ömer L. Barkan, "Daftar-ı Khākāni," *ibid.*, 81-83. The traditional character of Ottoman economic institutions as well as the general difference in economic interests between the military-administrative class—control of the land and its products—and the merchant community—manipulation of trade and financial affairs—is given in Halil Inalcik, "Capital Formation in the Ottoman Empire," *Journal of Economic History*, XXIX (1969), 97-140.

³⁹ Kritovoulos, 37, 83, 93, 140-41, 148-49, 184-85; Āshīkpāšāzāde, 133, 148; Ibn Kemal, 96-98, 101; Argenti, *Occupation of Chios*, I, 219.

the Muslim Empire, now in possession of an imperial capital, would do.³⁹ Certainly Christian states on the borders of the Ottoman Empire could expect battles, for the very origins of the house of Osman rested on the vigorous pursuit of the Holy War. And at Trabzon in 1461 the Conqueror confirmed that he warred for the sake of God and not the thrill of conquest.

The desire in all this exertion is not to conquer city and state, but to acquire God's eternal reward in the Holy War. To obtain His approval, it is a small thing if one endures a hundred thousand of the worst evils and afflictions in the way of the *ghazā* [war against the infidel], which is the highway to the paradises of Heaven.⁴⁰

Yet could a dynasty so conscious of its Turkish heritage also not be motivated by thoughts of world conquest?⁴¹ Ibn Kemal, the greatest of the early Ottoman historians writing on the fifteenth century, thought so; and in his grandiloquent statement on the worldly ambitions of Mehmet, he set down one of the main objectives of Ottoman naval policy during the entire period of expansion.

Like the world-illuminating sun he succumbed to the desire for world conquest and it was his plan to burn with overpowering fire the agricultural lands of the rebellious rulers who were in the provinces of the land of *Rūm* [Byzantine Empire]. He took with the hand of power and the grip of conquest one by one the cities and lands of the princes who were on the sea shores and it was his plan to flow over them in turn like a wave.⁴²

Even if early sultans derived great political force from old Turkish and Islamic imperial themes, let alone from Romano-Byzantine ideas, no ruler could create a stable empire in the Near East without major economic resources. When Mehmet occupied Istanbul he acquired a central imperial city in which he could concentrate the administration of a large state. From that city armies and navies now were sent out to seize the resources of the unbelievers. Assisted by inexpensive naval transportation, Ottoman administrators proceeded to use the economies of the conquered areas to support an imperial system whose base rested on an expanding interior economy.⁴³

Since a campaign to excise imperial opponents from the Levant, or for that matter to achieve world domination, implied the use of the sea, the Ottomans lost no time in seeking out knowledge of the maritime areas. Although the Palace School had been established by Mehmet the Conqueror to improve the general

³⁹ The impact of the Ottoman conquest on the Western world is covered in Robert Schwoebel, *The Shadow of the Crescent: The Renaissance Image of the Turk (1453-1517)* (Nieuwkoop, 1967), 1-81.

⁴⁰ Ibn Kemal, 196.

⁴¹ Ancient Turkish myths carry the idea that God had chosen the Turks to rule the world. Osman Turan, "The Ideal of World Domination among the Medieval Turks," *Studia Islamica*, IV (1955), 77-90. Pope Pius II (1458-64) believed that this was the objective of the Turks. Schwoebel, *Shadow of the Crescent*, 71-73.

⁴² Ibn Kemal, 180.

⁴³ Imperial naval policy aimed at securing control over the commercial wealth of the Levant. See Ibn Kemal, 105-06, 117, 165, 176, 180, 219-20, 287, 291, 309, 384, 388, 500; Kritovoulos, *passim*; William von Heyd, *Histoire du Commerce du Levant au Moyen Age* (Amsterdam, 1959), II, 1-313; and Carl M. Kortepeter, "Ottoman Imperial Policy and the Economy of the Black Sea Region in the Sixteenth Century," *Journal of the American Oriental Society*, LXXXVI (1966), 86-113.

quality of his slave corps, it also served as a gathering point for written and cartographical information describing all dimensions of the known world.⁴⁴ To advise him on worldly matters the sultan brought to Istanbul the Byzantine scholar George Amurazis who, by imperial command, drew up and commented upon a map of the world. Further evidence that naval affairs rated high attention is supported by the career of the Grand Vizier Mahmud Paşa.⁴⁵ Commander of the Ottoman fleet in the Black Sea campaign of 1461, in the Mytilene naval action of 1462, and in the Negroponte assault in 1470, this companion of the sultan also served as the patron of a historical poem, "Düstürnâme-i Enverî," part of which celebrated the fourteenth-century exploits of the pre-Ottoman corsair, Umur Bey.⁴⁶ Just as the grand vizier promoted the study of Turkish naval history within the terms of Ottoman culture, the sultan acquired Byzantine maritime experience by bringing a Greek from the island of Imbros, the historian Kritovoulos, into his administration.

If ideas of world conquest stirred Mehmet the Conqueror, his first naval campaigns showed that he would not proceed rashly but would advance from an interior position on land to eliminate Byzantine pretenders and to drive other Christian opponents from the shores of the Aegean and Black Seas. Severing at the Straits Black Sea Christians and their allies from Mediterranean support, the Conqueror launched land and sea assaults on Amasra and Trabzon in 1461 before attempting the purely naval conquest of Kaffa in 1475. Similarly the Ottomans conquered the Morea in 1460 before mounting their amphibious attack on Negroponte in 1470. Along the Anatolian coast, which had been Ottoman since the reign of Murad II, the Grand Vizier Mahmud Paşa quickly overran Genoese positions from the Dardanelles to the island of Mytilene in 1462, and Chios was placed under tributary conditions similar to those the Ottomans had imposed on Ragusa earlier in the fifteenth century.⁴⁷

The expulsion of Italians from their colonies and outposts in the Aegean and Black Seas plunged the Ottomans into their first long naval war. While the Genoese largely retreated from the eastern Mediterranean, the Venetians fought from 1463 to 1479 to reverse the slow Ottoman expansion from the interior of the Balkans and Asia Minor toward the ports and islands of the Aegean and Adriatic. Unable to develop sufficiently powerful Christian support and limited by its small

⁴⁴ This history of the Palace School is set forth in Barnette Miller, *The Palace School of Muhammad the Conqueror* (Cambridge, Mass., 1941). On the subject of Ottoman geographical knowledge in the fifteenth and sixteenth centuries see Abdülhak Adnan Adıvar, *Osmanlı Türklerinde İlim* (The Ottoman Turks and Science) (Istanbul, 1943), 55-79.

⁴⁵ Mahmud Paşa was a cousin of George Amurazis. Şehabeddin Tekindağ, "Mahmud Paşa," *İslam Ansiklopedisi* (hereafter *IA*) (Istanbul, 1940-), VII, 183-88.

⁴⁶ The translation of the portion on Umur Bey is in Enverî, *Le Destan D'Umur Pacha*, trans. Irène Mélikoff-Sayar (Paris, 1954).

⁴⁷ The size of the Ottoman fleets in the Trabzon, Negroponte, and Kaffa expeditions, as reported in Ottoman sources, runs between one hundred and three hundred ships. Again these figures include no breakdown according to technical categories. See Ibn Kemal, 181, 285, 385; 'Aşîkpâşâzâde, 148, 175; Kritovoulos, 165. The position of Ragusa in the Empire is developed in Nicolaas H. Biegun, *The Turco-Ragusan Relationship* (The Hague, 1967), 26-59; and Bariša Krekić, *Dubrovnik (Raguse) et le Levant au Moyen Âge* (Paris, 1961), 39-65.

population, the Venetians found it difficult to use their superior naval ability to stop the Muslims' simultaneous advance on land and sea. On the other side of the conflict the Ottomans, fearful of an open sea battle with the Italians, kept their galleys well within range of their own lands so that Muslim numerical superiority on land could prevent a successful Venetian admiral from taking Ottoman territory. Before 1480 the war on sea, therefore, worked itself out in a series of raids and indecisive confrontations in which the Muslim fleet remained virtually untested in the art of large-scale galley warfare.⁴⁸

In the last year of the Conqueror's life the Ottoman threat to the Italian naval position in the Levant reached a critical stage. Italians, after the end of the Venetian-Ottoman war in 1479, still remained in control of fortified stations and islands—Modon, Koron, Crete, Cyprus, Rhodes—from which they could both dominate Near Eastern trade routes and attack Ottoman shores at will. Now fearless of Christian naval opposition, the Conqueror sent his galleys out during the spring and summer of 1480 to overturn Christian command of the northeastern Mediterranean by seizing the island of Rhodes and the southern Italian city of Otranto. But the Knights of St. John defended Rhodes with great spirit and forced the Ottoman fleet to retire without a victory in the fall of that year. Meanwhile the northern Ottoman expedition established a beachhead in southern Italy. The death of Mehmet in 1481, however, unleashed all the problems of a contested succession for the Ottoman sultanate. The new ruler, Bayazid II, facing internal opposition from his younger brother Jem, did not risk his fleet in defense of Otranto when Christian galleys appeared in the Adriatic during the summer of 1481. Lacking adequate naval support, the expedition in Italy collapsed and joined the defeat at Rhodes to mark the limits of Ottoman naval operations in the eastern Mediterranean during the reign of Mehmet the Conqueror.⁴⁹

While Bayazid II engaged in an internal reorganization of the Empire between 1481 and 1495 in order to consolidate his position, the need for a stronger Ottoman navy rose. War with the Mamluks on the Taurus Mountain frontier from 1485 to 1491 strained the supply system of the Ottomans. The sultan, therefore, requested from the Venetians the use of the port of Famagusta on the island of Cyprus in order to support his border forces from the sea. Not only did the Venetians reject the sultan's petition, but they emphasized the naval vulnerability of the Ottomans by sending their own fleet to Cyprus.⁵⁰ In the last quarter of the

⁴⁸ Venetian tactics at Negroponte are covered in William Miller, *The Latins in the Levant* (New York, 1908), 471–81. Thiriet details the naval situation up to 1481 in *Romanie vénitienne*, 387–94, 443.

⁴⁹ Ibn Kemal shows that the Rhodes and Otranto campaigns coincided with land operations against the Mamluks (pp. 500–10, 528, 542, 544). For another view of the Otranto operation, see Babinger, *Mahomet II*, 477–97. The revolt of Jem is summarized in Halil İnalcık, "Djem," *El²*, II, 529–31.

⁵⁰ Selahattin Tansel lists the following reasons for the wars between the Ottomans and the Mamluks: the development of political connections between the Muslim states in India and the Ottomans during the reign of Mehmet the Conqueror, the failure of the Mamluks to recognize pub-

fifteenth century a cry for aid from the Muslims in Spain reached the Ottomans.⁵¹ Here also the champions of the Holy War⁵² could do nothing without an effective navy. Closer to the Aegean, the encirclement of the Greek peninsula by Venetian bases reminded the Ottomans that the eastern frontier also remained insecure.

Recognizing the deficiencies of his military establishment, Bayazid II began to acquire experienced naval officers for an expanded fleet. Just as early sultans had drawn able men from the Christian-Muslim land frontier, so Bayazid II went to the naval border between Islam and Christendom for able galley captains. There the Muslim corsair was the equivalent of the fifteenth-century Balkan *ghāzī*. Like their land counterparts, the naval frontiersmen were products of a brutal naval life in which failure often meant death while success could lead to wealth and power. To profit from the crude but effective training system of the sea *ghāzīs*, Bayazid II summoned to Istanbul corsair captains, the most famous of whom were from the shores and islands of the Aegean, and offered them positions in the imperial naval organization. By 1495 this procedure attracted into Ottoman service sea captains such as Kemal Reis, Burak Reis, and Piri Reis.⁵³

Recruitment of the corsairs quickly raised the technical competence of the Ottoman navy. Experienced in open-sea combat, the corsairs possessed both the skill and the desire to lead Ottoman fleet units against the Christians. Moreover, their wide raiding activities brought into the Ottoman naval institution a wealth of information concerning the Mediterranean. Later on in the reign of Selim the Grim this naval and political intelligence was summarized by Piri Reis in the *Kitab-ul Bahriye* (Book of Sea Lore).⁵⁴ Finally, the infusion of the corsairs took place during a period of increased galley construction, which meant that Ottoman

lily the power of the Ottoman sultan, the use of Egypt as a political base by Bayazid II's opponents, the refusal of the Mamluk sultans to allow Ottomans to participate in the operation of the Pilgrimage to the Holy Cities of Mecca and Medina, and finally the appearance of border disputes between Ottomans and Mamluks in northern Syria. *Sultan II Bayezit'in Siyasi Hayati* (The Political Life of Sultan Bayazid II) (Istanbul, 1966), 93–100. From the Mamluk side, the same information is included in Ibn Iyas, *Histoire des Mamlouks Circassiens 872–906*, trans. Gaston Wiet (Cairo, 1945) II, *passim*. For the communication with the Turko-Muslim frontier state in India, the Bahmani Sultanate, see Gelibülili Mustafâ 'Âli Çelebi, "Küh-ul ahhâr" (The Totality of Information), Istanbul University Library, TY 5959, IV, fol. 140^v; Ibn Kemal, 544–45; and Ahmed Feridun Bey, *Münşâ'ât-us-Salâtin* (The Writs of Sultans) (Istanbul, 1275/1858), I, 258–60. The ambassador to the Ottomans is given as Kh'āca Cemāl ad-Dīn Hasan (p. 259, line 22).

⁵¹ Mohammed Abdullah Enan, *Nihāyat al-Andalus* (The End of the Moorish Empire in Spain) (Cairo, 1958), 204–05; James T. Monroe, "A Curious Morisco Appeal to the Ottoman Empire," *Al-Andalus*, XXXI (1966), 281–303.

⁵² Following the conquest of Istanbul in 1453 Ottoman sultans claimed leadership in the Holy War. Halil Inalcik, "Padişah," *IA*, IX, 491–95.

⁵³ All three among the most famous of the early Ottoman naval commanders. For Kemal Reis, whose birthplace is disputed, but whose naval experience began in the Aegean, see Hans-Albrecht von Burski, *Kemal Reis: Ein Beitrag zur Geschichte der türkischen Flotte* (Bonn, 1928), 1–25. The partial English translation of the seventeenth-century naval history of the Ottomans by Hajji Khalfa deals with Burak Reis. *History of the Maritime Wars of the Turks*, trans. James Mitchell (London, 1831), 19–23. Abundant references to original sources on the life of Piri Reis are in Fuad Ezgü, "Piri Reis," *IA*, VIII, 561–65. Finally, Gelibolu Mustafa 'Âli describes how corsairs began their careers on the shores of the Ottoman Empire. *Mei'ād-id-ün-Nef'is fi Kavā'id-il-Mecālis* (Examples of Etiquette for the Guidance of Society) (Istanbul, 1956), 54–56.

⁵⁴ Text in Piri Reis, *Kitab-ı Bahriye*, facsimile ed., Publications of the Turkish Historical Society, no. 2 (Istanbul, 1935).

arsenals also benefited from the knowledge of seamen who lived by constant warfare.⁵⁵

Since the addition of the sea *ghāzīs* to the Ottoman fleet marked such an important turning point in the evolution of the Ottoman seaborne empire, it is worthwhile examining the motives that encouraged Muslims to take to the sea as privateers. From the time of the earliest Islamic conquests Muslims had carried the Holy War against the infidel from land to sea where the institution of the Jihad also applied. Stimulated by religious duty and a military tradition enhanced by an imaginative "Jihadist" literature,⁵⁶ Muslim warriors on the sea frontiers responded to the call of the Holy War in increasing numbers as the Ottoman Empire expanded along the shores of the Mediterranean. Although the Islamic world had no phenomenon quite like the legend of Prester John, reports from distant frontiers concerning the oppression of Muslims attracted seagoing *ghāzīs*.⁵⁷ Behind the corsairs, the Ottoman sultan, who claimed to be the champion of the Islamic world in the Holy War, added the support of a great state. For those corsairs who won fame in frontier wars, the Ottoman administration, by the late fifteenth century, could offer state positions that went well beyond the rewards of a privateer. If the glorious career of a warrior for the faith yielded insufficient motivation, the capture of infidel prizes, the seizure of rich frontier regions, and the prospect of adventure stimulated many a sailor from poor and probably overpopulated coastal regions to become a corsair.⁵⁸

Confident that by the summer of 1499 the Ottoman fleet could compete with Christian flotillas, Bayazid II commanded his viziers to implement the naval policy laid down by his father. The galleys of the Ottomans, including two ships of extraordinary size, sailed toward the Greek peninsula in order to make the sea the frontier. The ensuing battles off the southwest portion of the Morea resulted in the defeat of the Venetian fleet and the Ottoman conquest by 1503 of Lepanto, Modon, Koron, Navarino, and Durazzo. Reversing centuries of frontier history, the Ottomans had achieved naval supremacy in the eastern Mediterranean. But the impact of the Muslim victory would be felt far beyond the Levant, for no single Christian state could parade the naval strength now available to Bayazid II. Moreover, many of the important northwestern ports connected with the rich Indian spice trade fell into the hands of the Ottomans at the same time that Portuguese caravels reached the trading cities on the west coast of India.⁵⁹

⁵⁵ Kemāl-pāšāzāde (Ibn Kemal) reports that Bayazid II's policy was to employ ship captains who were experienced in all sea matters. "Tevārīḥ-i Āl-i 'Osmān, VIII Deftēr" (The History of the Ottoman Dynasty, Book 8), Millet Kütubhanesi, Istanbul, MS no. 32, fol. 65^{a-b}.

⁵⁶ The ballad of Umur Paşa is only one example of a much wider body of literature. Alessio Bombaci, *Histoire de la Littérature Turque*, trans. Irène Mélikoff (Paris, 1968), 259–66.

⁵⁷ Burski, *Kemal Reis*, 20–24; Andrew C. Hess, "The Moriscos: An Ottoman Fifth Column in Sixteenth-Century Spain," *AHR*, LXXIV (1968), 7–8.

⁵⁸ The sixteenth-century Spaniard, Fray Diego De Haedo, remarked that Ottoman corsairs came to North Africa "... con tan codicia como los españoles pasamos al las minas de las Indias" (with as much greed as the Spanish we send to the mines of the New World). *Topografía e Historia General de Argel* (Madrid, 1927), I, 220–21.

⁵⁹ The Ottomans built two large Mediterranean sailing ships armed with cannons and other war

Of these two events, both of which marked major turning points in the history of the Near East, the victories of the Ottomans held more immediate consequences for the Muslim rulers of the Arab lands. For over a quarter century political competition between Ottomans and Mamluks had steadily risen in intensity. Now it seemed only a matter of time before the superior naval strength of the Ottomans would encourage the sultan to eliminate his imperial opposition in the orthodox Muslim community, to acquire the prestige in the Islamic world of protecting the Holy Cities of Mecca, Medina, and Jerusalem, and to control the lands and trade of Egypt and Syria.⁶⁰

While Venetians adjusted to the supremacy of Ottoman sea power, the Mamluks found themselves ringed by dangerous, external powers. Having defeated the Venetians, the Ottomans could now intensify their frontier operations in the Taurus Mountains unhindered by a naval threat from the Adriatic. But equally dangerous for the Syrian domains of the Mamluks were the movements of the Safavid warbands in the hills of Anatolia.⁶¹ If the perils in the north seemed truly ominous, the sudden news that Portuguese ships had arrived in the Indian Ocean added even greater uncertainty to the defensive requirements of the beleaguered Mamluk kingdom.⁶²

When in the first decade of the sixteenth century the Portuguese made their presence known to Muslim merchants trading in the seas off South Arabia, the Mamluks were forced, for reasons in addition to the protection of trade, to make dangerous policy decisions regarding their southern frontier. Not only were the trading routes to India exposed to attack, but the Holy Cities in Arabia also awaited the protection that had bolstered the political position of the Mamluk emirs in the Islamic world. But how would the Mamluks, crippled in naval matters by both the absence in Egypt of such essential war materials as wood, iron, and copper, and the aristocratic contempt of horsemen for anything maritime, be able to expel the technically powerful Portuguese fleet? Cooperation with the Safavids would yield little naval assistance and would associate, in a culture where politics and religion were intimately mixed, the Protectors of the Holy Places with the unorthodox. On the other hand to accept aid from Bayazid II would only confirm Ottoman superiority over a rival Muslim military government. Moreover, it sug-

material. Sa'd ed-Dīn (Khoja Efendi), *Tāj-ut-Tevāriḫ* (The Crown of Histories) (Istanbul, 1280–63), II, 88–89. But what was most striking about the navy under Bayazid II was its increased size. See above, n. 30; and the end of the fifteenth-century estimates in Marino Sanuto, *I Diarii*, ed. Nicolo Barozzi *et al.* (Venice, 1879–1903), I, cols. 398–99, 323; II, cols. 568–70; III, cols. 1348–49. An estimate of Ottoman sea power in relation to other Mediterranean states after 1500 is contained in Vitorino Magalhães-Godinho, “A viragem mundial de 1517–1524 eo o império português,” in *Ensaio Sobre História de Portugal* (Lisbon, 1968), II, 141–53.

⁶⁰ Sa'd ad-Dīn describes the frontier battles between the Ottomans and the Mamluks from 1485 to 1491 as a war for castles that were the “keys to the Arab lands.” *Tāj-ut-Tevāriḫ*, II, 51. Tansel presents evidence that arguments were being made at Bayazid II's court for the conquest of Arab territories. *Sultan II Bayezit'in Siyasi Hayati*, 97–98.

⁶¹ Ibn Iyas, *Journal d'un Bourgeois du Caire*, trans. Gaston Wiet (Paris, 1955–60), I, 35–36.

⁶² Ibn Iyas notes the division of Mamluk forces to cover the northern and southern dangers to the Empire. *Ibid.*, 79.

gested that the Ottomans might absorb the Mamluk Empire just as they had taken the Muslim states in Anatolia during the fifteenth century. Between 1506 and 1509 the Mamluks made their choices and, in competition with the Portuguese, established the outline of a frontier conflict in the Indian Ocean: the Mamluks fortified the port for the Holy Cities, Jedda; in 1507 the Portuguese seized the island of Socotra just outside the Bab el-Mandab; and the Mamluk sultan, after the 1509 defeat of his own armada by the Portuguese at Diu, requested naval assistance from the orthodox Ottoman Empire.⁶³

Following Francisco de Almeida's victory over the Mamluk fleet off Diu in 1509, frontier warfare between Muslims and Christians on the sea involved the Ottomans in an Indian Ocean naval war. Under the leadership of a new viceroy, Afonso de Albuquerque, the Portuguese attempted to cut the ancient trade routes that ran from India north through the Persian Gulf and Red Sea to the cities of the Levant. Vividly impressing Muslims with his seagoing fire power, Albuquerque seized ships and bombarded ports along the southern rim of the Islamic world, compelling Muslim rulers and merchants⁶⁴ to send urgent appeals for aid to a Mamluk sultan whose navy, after 1509, was no match for the Portuguese. Already, however, a Mamluk ambassador to Bayazid II had succeeded in a mission directed at acquiring the materials for a new fleet. With deceptive ease, naval supplies and seagoing military men began to work their way south from Anatolia toward the Red Sea port of Suez.⁶⁵ Undoubtedly the Mamluks knew that Ottoman assistance had its price, but by 1513 the audacity of the Portuguese had made considerations other than the development of a Red Sea fleet seem less important. Although Albuquerque, with a flotilla of some twenty ships,⁶⁶ failed to take Aden in 1513, he subsequently sailed into the Red Sea where his seizure of the island of Kamarān placed the port of the Holy Cities, Jedda, within reach of infidel cannons.⁶⁷ Meanwhile the construction of a second Mamluk fleet at Suez proceeded with all the character of an Ottoman expedition. Under the direction of Salmān Reis, a Muslim captain from the eastern Mediterranean,⁶⁸ laborers translated the

⁶³ On the famous Portuguese policy see Afonso de Albuquerque, *Commentaries*, trans. Walter de Gray Birch, Hakluyt Society Publications, old ser. (London, 1875-84), I, 20. The Muslim reaction before the conquest of Egypt in 1517 is in R. B. Serjeant, *The Portuguese off the South Arabian Coast* (Oxford, 1963), 160-62; and Ibn Iyas, *Journal*, I, 152-53.

⁶⁴ Ibn Iyas supports Portuguese chroniclers who claim that the initial opposition to the Portuguese came from the Muslim rulers and merchants located along the rim of the Indian Ocean. *Journal*, I, 176. See also Caspar Corrêa, *Lendas da Índia* (Lisbon, 1858-66), IV, 428-33, 748-50; and Damião de Góis, *Crônica do Felicíssimo Rei D. Manuel* (Coimbra, 1949-54), IV, 33.

⁶⁵ Burski, *Kemal Reis*, 66-75.

⁶⁶ Serjeant, *Portuguese off the South Arabian Coast*, 47 n.F, 169.

⁶⁷ There was a public reaction in Cairo. Ibn Iyas, *Journal*, I, 288-91.

⁶⁸ The Mamluk sultan, Kānsūh al-Ghawrī, appointed Salmān Reis as the admiral of the fleet that was sent from Suez in 1515 to defend the Red Sea ports against the Portuguese. According to João de Barros e Diogo de Couto, Razez Soleimāo (Salmān Reis) was born into a Turkish family on the island of Mytilene. *Da Ásia* (Lisbon, 1777-88), decade III, bk. I, chap. II, 31-32. After acquiring naval experience in the eastern Mediterranean, he entered the service of the Mamluk sultan at the turn of the sixteenth century. 'Abdallah Muḥ. bin 'Omar al-Makkī, al-Āṣafī, Ulughkhānī (Hajji ad-Dabir) states that Salmān Reis led a group of corsairs who won fame in the Holy War, but says nothing about his attachment to the Ottoman Empire before 1517. *An Arabic History of Gujarat* (hereafter Ulughkhānī), ed. E. Denison Ross (London, 1910-28), I, 218-19. Ibn Iyas, however,

raw materials collected by the grace of an Ottoman sultan into the form of a galley fleet. Not only did the subjects of the Ottoman sultan direct the construction of the second Mamluk armada, but they also staffed its ranks of galley captains and provided up to two thousand arquebusiers⁶⁹ for its amphibious unit. Albuquerque knew very well where this naval experience came from when in 1513 he looked for the fleet of *os Rumes*.⁷⁰

Even before the Mamluk fleet sailed from Suez to protect the interests of the Muslim community in the Red Sea regions, the 1514 Ottoman victory in north-western Azerbaijan over the Safavids of Persia suddenly twisted the attention of the Mamluk sultan from south to north. With the forces of the Safavids in disarray after the battle of Chaldiran, and with Mamluk military and naval failings well publicized, Selim the Grim announced to the Muslim world the political superiority of the Ottoman family.⁷¹ These new claims indicated that Syria and Egypt would be the next targets of Ottoman expansion, and the news in 1515 that Selim had prepared a fleet, reported to be as large as four hundred ships, further confirmed Mamluk fears. One year after the defeat of the Safavids the rulers of Egypt began defensive preparations against an attack from the north.⁷²

While the Mamluks strengthened their highly conservative military establishment, Salmān Reis introduced Ottoman influence into South Arabia. Like the

states that the Mamluks charged Salmān Reis, an Ottoman (*al-Uthmānī*, in the Arabic text), with the construction and operation of the 1515 Mamluk fleet. *Journal*, I, 340. Serjeant, translating South Arabian texts, records Salmān Reis as *al-Rūmī*: a sailor from the territories of the Byzantine Empire. *Portuguese off the South Arabian Coast*, 48–50. Since the arrival of Salmān Reis in Egypt coincided with the Ottoman military aid program, Ibn Iyas' report is probably correct. That Salmān Reis quickly entered the Ottoman navy after the conquest of Egypt in 1517 is clear from all sources. Serjeant, *Portuguese off the South Arabian Coast*, 50–51; L. O. Schuman, *Political History of the Yemen at the Beginning of the 16th Century* (Amsterdam, 1961), 17–38, 77–79; Feridun, *Münş'ât*, I, 491, 498; and De Barros, *Asia*, decade IV, bk. I, chap. IV, 44–46.

⁶⁹ The presence of Ottoman arquebusiers in the Mamluk fleet is discussed by Ayalon, *Gunpowder and Fire Arms*, 78–82; Schuman, *Political History of the Yemen*, 20, 76–78; and Ibn Iyas, *Journal*, I, 337–40. In his review of David Ayalon's book, Halil Inalcik notes that Ottoman documents cover the appointment of ship captains to construct war galleys in Egypt for the Mamluks from before 1512. *Belleten*, XXI (1957), 501–08. Records of other forms of military aid are in Ibn Iyas, *Journal*, I, 177–79, 185–86, 195–97, 267; and De Barros, *Asia*, decade II, bk. I, chap. III, 29.

⁷⁰ This term has received various interpretations. It is derived from the Arabic *al-Rūmī*, which generally described someone who came from the territories of the now deceased Byzantine Empire. When used in a military context during the sixteenth century the word usually implied a connection with the Ottoman Empire, renegade Christians, or other non-Muslims, being known by their religion. In Portuguese, Arabic, and Ottoman sources the term was also employed to describe the seamen who were enlisted by the Mamluks and the Ottomans to counter the Portuguese. Albuquerque (*Commentaries*, II, 60–80) and Corrêa (*Lendas da Índia*, I, 741–44) apply this term to sailors who fought in the Egyptian fleets during 1508–09. The definitions of De Barros (*Asia*, decade IV, bk. IV, chap. XVI, 459–60) and Couto (*Asia*, I, pt. 2, pp. 264, 395) leave little doubt that *os Rumes* were sailors from the coastal regions of the Ottoman Empire. For Arabic sources supporting the same general definition, see Serjeant, *Portuguese off the South Arabian Coast*, 48 n.7. From Ottoman sources, the 1525 report of Salmān Reis on Turkish naval strength in Jedda lists one thousand *gemici Rum yığitleri* (brave sailors from Ottoman territories). Fevzi Kurtoglu, "Meşhur Türk Amiralı Salmān Reisin Layihası" (A Document by the Famous Turkish Admiral Salmān Reis), *Deniz Mecmuası* XLVII (1935), 68.

⁷¹ In his correspondence with other Muslim rulers Selim the Grim used titles that asserted an Ottoman protection over the Holy Cities. Halil Inalcik, "Padişah," *IA*, IX, 491–95; and Cutb ed-Din, *Geschichte der Stadt Mekka und ihres Tempels*, ed. Ferdinand Wüstenfeld, in *Die Chroniken der Stadt Mekka* (Leipzig, 1858–61), III, 278. For the reaction of the Mamluks see Ibn Iyas, *Journal*, I, 356–57, 371–74, 401–02.

⁷² Ibn Iyas, *Journal*, I, 435, 440–46.

Portuguese, the Turks concentrated their attention on the South Arabian shore in order to control the naval traffic through the Bab el-Mandab. Taking Zabīd in Yemen during 1516, the Mamluk armada then moved on to attack the strategically located port of Aden.⁷³ Although the Turks failed to breach the walls of that city, they left a far different impression in Muslim histories of South Arabia than did their equally unsuccessful predecessors, the Portuguese. Recording the Muslim Turks as the representatives of an imperial expansion that aimed to administer lands and peoples rather than hold fortified commercial stations, the South Arabians marked the arrival of the Mamluk fleet in 1516 as the beginning of the Ottoman domination in Yemen.⁷⁴

While the land units of the Turkish force consolidated their position in Yemen, Salmān Reis returned to Jedda where in 1517 he confronted the new technology of the Atlantic sailing ships with the naval equipment of the eastern Mediterranean. Under orders to destroy the fleet of the *Rumes*, the Portuguese admiral, Lopo Soares, sailed his armada through the Bab el-Mandab to engage the galleys of Salmān Reis. The Turkish admiral responded to the appearance of the Portuguese fleet with tactics that not only neutralized the fire power of the Portuguese sailing ships but also emphasized the cultural differences between the two empires. Arranging his galleys in a tight group within the narrow, reef-bound harbor of Jedda where he could depend on artillery support from land, Salmān Reis waited for the Portuguese either to commit themselves to what amounted to a land battle in which the more numerous and acclimated Muslims held the advantage, or to retire when their limited ship-borne supplies ran out. Lopo Soares, following the orders of his king to fight only on sea, succumbed to Turkish tactics as desultory combat, disease, and depleted supplies weakened his fighting forces.⁷⁵

Defeated in their attempt to destroy the fleet of the *Rumes* and dominate the seas around Arabia solely through naval power, the Portuguese turned to diplomacy. Albuquerque was already aligned against the Muslim 'Ādil-shāh dynasty of Bijapur through friendly contacts with the Hindu rulers of Vijāyanagar, and he anticipated the consequences of the Ottoman victory at Chaldiran in 1514 by writing to Manuel I for permission to supply Shah Ismail⁷⁶ with cannons so that the

⁷³ For the 1516 campaign of Salmān Reis see Schuman, *Political History of the Yemen*, 20–24; Serjeant, *Portuguese off the South Arabian Coast*, 16, 48; and Ulughkhānī, I, 43, 46.

⁷⁴ Serjeant, *Portuguese off the South Arabian Coast*, 48; Schuman, *Political History of the Yemen*, vii–viii.

⁷⁵ Although historians of both cultures dislike the lack of a clear-cut victory, all sense the importance of the event that marked the beginning of Ottoman-Portuguese frontier warfare. De Barros, *Asia*, decade II, bk. I, chap. IV, 39–54; Fernão Lopes de Castanheda, *História do descobrimento e conquista da Índia pelos Portugueses* (Lisbon, 1833), IV, 19–26; Corrêa, *Lendas da Índia*, II, 494–98; Schuman, *Political History of the Yemen*, 31–33; Serjeant, *Portuguese off the South Arabian Coast*, 50. On how galleys should attack sailing ships see Kâtib Çelebî, *Tuhfet-ul Kibâr fi Esfâr-ul-Bihâr* (The Prizes of the Great in the Campaigns on the Seas) (Istanbul, 1329/1911), 150–51, 161.

⁷⁶ Albuquerque, *Commentaries*, IV, 182–84. The alliance lasted well into the sixteenth century. Rodrigo José de Bocarro, *Decada 13 da historia da Índia*, ed. Rodrigo José de Lima Felner (Lisbon, 1876), pt. 2, p. 369. On the international struggle, which involved the Ottomans, over the Indian border state ruled by the 'Ādil-shāh dynasty see Firishṭa, *Tārīkh-i Firishṭa* (Firishṭa's History) (Lucknow, 1321–23/1903–05), II, 2, 11, 13, 14. For the English translation, see Muhammad Kāsim

Safavids might divert the rising power of the Ottomans from India. In a similar manner, Francisco Alvares, after the failure of the Portuguese Red Sea campaign in 1517, mobilized the support of Christian Ethiopia against the advance of the Ottomans into Red Sea regions.⁷⁷ By the first quarter of the sixteenth century the efforts of the Christians produced an alignment of states—Ethiopia, Persia, and South India—hostile to the expansion of the Ottomans south of Egypt.

Just as the battle at Jedda reflected long-standing conditions of Muslim-Christian naval warfare, albeit altered in the level of technology, so also did the 1517 conquest of Egypt bring the Portuguese and Ottoman empires together under equally familiar circumstances. Possession of Arab lands meant that the Ottoman sultan acquired the duty of protecting the Holy Places and, therefore, of securing Red Sea defenses. True to its *ghāzī* tradition, the Ottoman dynasty also became the Islamic champion for the besieged Muslim states and trading posts east of Suez upon which Portuguese imperialism impinged. But nowhere were the old differences between Near Eastern land empires and the commercial maritime states from the Christian world better mirrored than in the conflicting imperial claims of the two empires in contact off Arabia. In 1499 Manuel I asserted that he was “Lord of Guinea and of the Conquests, *Navigations*, and *Commerce* of Ethiopia, Arabia, Persia, and India.”⁷⁸ On the conquest of Egypt, Selim the Grim announced:

Now all of the *territories* of Egypt, Malatiya, Aleppo, Syria, the *city* of Cairo, Upper Egypt, Ethiopia, Yemen, the *lands* up to the borders of Tunisia, the Hijaz, the *cities* of Mekka, Medina, and Jerusalem, may God increase the honoring and respecting of them completely and fully, have been added to the Ottoman Empire.⁷⁹

Before his return to Istanbul in 1518 Selim the Grim collected naval intelligence and ordered the construction of a new fleet.⁸⁰ In 1517, therefore, Piri Reis presented his famous map of the New World to the Sultan, giving the Ottomans, before many European rulers, an accurate description of the American discoveries as well as details about the circumnavigation of Africa. A year later Salmān Reis, who had fought the Portuguese and made prisoners of their men, added his experience to the Ottoman camp. But before Selim could exercise a new fleet, or act on the appeal of ‘Alī Ekber to conquer China, his short reign came to an end with his death in 1520.⁸¹

Firishtah, *History of the rise of the Mahomedan power in India till the year A.D. 1612*, trans. John Briggs (London, 1829).

⁷⁷ Francisco Alvares, *The Prester John of the Indies*, trans. Lord Stanley of Aldrley, revised C. F. Beckingham and G. W. B. Huntingford, Hakluyt Society Publications, new ser. (Cambridge, 1958–61), I, 287, 305–06; II, 416–17.

⁷⁸ Quoted in Góis, *Crónica*, I, 114 (my italics).

⁷⁹ Quoted in Feridun, *Münşâât*, I, 429 (my italics).

⁸⁰ Lutfi Paşa, *Tevâriḫ-i ‘Alī ‘Osmān* (The History of the Ottoman Dynasty) (Istanbul, 1341/1922–23), 304–05, 304 n.4.

⁸¹ Paul Kahle, *Opera Minora von Paul Kahle*, ed. Mathew Block (Leiden, 1956), 247–65. Translations into English of the Ottoman comments on Piri Reis’ map are in Afet Inan, *The Oldest Map of America, Drawn by Piri Reis*, trans. Leman Yolaç (Ankara, 1954), 28–34. For references to the *Hiṭây-nâme* (A History of China) of Seyyid ‘Alī Ekber Hiṭâyî, see Joseph Matuz, *L’ouvrage de Seyfi Çelebi* (Paris, 1968), 20, 23–25.

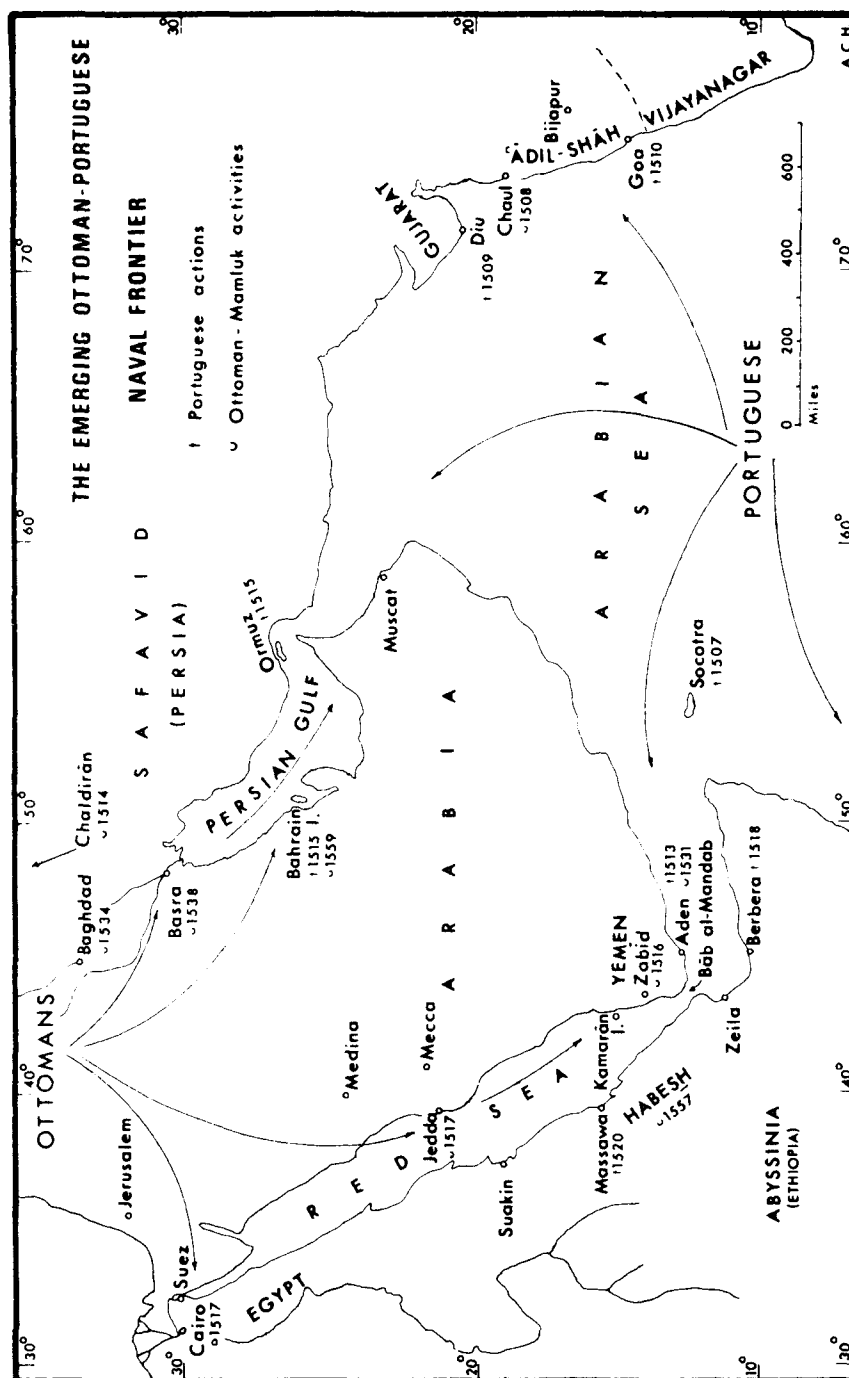
During the three years between the accession of Suleyman the Magnificent and the end of the Rhodes campaign in 1523, the Ottomans used their navy to consolidate the conquests of Selim the Grim. While the Portuguese campaigned off Aden in 1520 with approximately twenty ships, the Ottomans, under the command of Husayn Bey, strengthened the Turkish position around the city of Zabīd in Yemen.⁸² To the north a Syrian revolt in 1520–21 exposed the uneasy Ottoman control of the Arab provinces to the rulers of the Christian-held islands—Rhodes, Cyprus, Crete—just off the Anatolian and Syrian coasts. Re-adopting the policy of the Conqueror in order to strengthen his administration of the Arab provinces, Suleyman launched the Rhodes campaign in 1522. Ottoman chroniclers, underscoring the importance of naval communications, justified the war as necessary for the protection of the sea routes both for the Pilgrimage to the Holy Cities and for Muslim trade. But the court historians who prepared this Islamic version of history, with its stress on the duty of a Muslim ruling class to protect religion and urban economic interests, did not elaborate on the dramatic new flow of tax revenues from Egypt to the Ottoman treasury in Istanbul.⁸³

Although the seizure of Rhodes seemed to preserve the continuity of Ottoman naval expansion, the intervening conquest of Belgrade in 1521 indicated that the sultan had now turned away from the sea frontiers. Would the Ottomans continue to expand on two frontiers? The question appeared to be unsettled in 1523. Not until 1525 did the imperial commanders reach agreement on where the Empire ought to concentrate its military resources. Another rebellion in Egypt during 1523–24 had postponed the decision because the sultan was forced to send his most important servant, Grand Vizier Ibrahim Paşa, to Cairo in order to restore control. In the interim the Janizaries, apparently encouraged by high state officials, rebelled over the military inactivity of the sultan. Upon his return to Istanbul in the fall of 1525, Ibrahim Paşa brought to Suleyman's attention a revised copy of Piri Reis' *Book of Sea Lore*. Along with its coverage of the Mediterranean and Black Seas, the new recension of this naval handbook featured a detailed description of the voyages to the New World and Indian Ocean, the trading activity, and the naval technology of the Portuguese.⁸⁴ Summing up, in Ottoman style, an appeal for continued naval expansion, the poetical introduction of the manuscript con-

⁸² Serjeant, *Portuguese off the South Arabian Coast*, 18; Fevzi Kurtoglu, *Turgut Paşa* (Istanbul, 1935), 490–509.

⁸³ Matrâkçı Naşuh, "Dâstân-i Sultân Süleymân" (A History of Sultan Suleyman), TKS, R. 1286, fols. 54^b–55^b; Sivâsi, "Süleymân-nâme" (A History of Sultan Suleyman), *ibid.*, H. 1340, fols. 31^a–44^a. A study of the Ottoman revenues from Egypt is in Stanford J. Shaw, *The Financial and Administrative Organization and Development of Ottoman Egypt 1517–1798* (Princeton, 1962), 283–312; the research presented here is upheld by Sanuto, *Diarii*, XLI, cols. 534–35.

⁸⁴ The poetical introduction of Piri Reis' second version of his *Book of Sea Lore* also contains a history of Portuguese naval explorations from the age of Prince Henry the Navigator and details concerning the world's known seas. Even greater information on the Portuguese Empire in the East, on the size of Portuguese garrisons in Asia, on the strategic and economic objectives of the Christians in the East is in the 1525 report of Salmân Reis. Kurtoglu, "Meşhur Türk," 67–73. For an analysis of the poetical introduction on pages 7–85 in the facsimile edition of the *Book of Sea Lore*, see *ibid.*, i–lv; and Paul Kahle, "The Turkish Sailor and Cartographer," in *The Proceedings of the Pakistan History Conference*, ed. S. Moinul Haq (Karachi, 1956), 101–11.



tained the powerful charge that the Ottomans ought to be ashamed of allowing the Portuguese to approach so close to the Holy Cities.⁸⁵

In addition to the new information from Piri Reis, the sultan received a detailed military report from Salmān Reis on the strength and objectives of Ottoman and Portuguese forces operating in the Indian Ocean. Salmān also called for further action against the Portuguese. He argued that the sultan should expand military operation in the Red Sea and Indian Ocean in order to acquire the taxes from the agricultural lands of Yemen and from the commercial trade that passed through the Red Sea and Indian Ocean ports. He noted, however, that the Portuguese, from their fortified ports on the Strait of Malacca, on the islands of Sumatra and Ceylon, and along the coast of India and Persia, had now diverted to Portugal much of this Indian Ocean trade that had once provided large tax returns for Egypt. To remove the infidels, and other Muslim opposition, to the imposition of an Ottoman tax system, the Muslim frontier captain proposed to raze the forts of the sultan's opponents with the existing naval forces stationed in the Red Sea. According to Salmān, the main elements of his galley squadron, which was based at Jedda, consisted of eight galleys, three galliots, one thousand sailors from *Rum*, and various siege cannons. Planning on the basis of eastern Mediterranean galley warfare, the Ottoman captain did not propose to undertake open sea battles with the Portuguese sailing ships. Rather, the galleys of the sultan were to be used for the standard Ottoman naval campaign, the climax of which usually involved an assault on a fortified position.⁸⁶

By the end of 1525 the Ottomans came to a decision on the priorities of their various frontiers. Increasing the number of galleys over the figures in Salmān's report, they determined that some twenty-five ships and four thousand men would best advance the sultan's interests in South Arabia. But this modest Red Sea squadron represented no major commitment of Ottoman forces: the main decision in Istanbul had gone against the navy and the economic interests that might have benefited from Ottoman campaigns in the Indian Ocean. In 1526 Suleyman the Magnificent redirected the energies of the Ottoman Empire into a series of land campaigns that would shake the walls of Vienna. Reflecting the consequences of its political defeat at the hands of military and administrative groups seeking more land, the naval organization languished until the Habsburg assault on the Morea in 1532 ushered in a new period in Ottoman naval history.

The conquests of Selim the Grim completed the basic structure of the Ottoman seaborne empire. On the accession of Suleyman the Magnificent, the Ottomans ruled coastlines and islands stretching from the Crimea to Yemen. In the north the Black Sea had lain secure under Ottoman domination since the reign of Mehmet the Conqueror. To the west and south two sea frontiers, or zones of naval

⁸⁵ Piri Reis, *Kitab-ı Bahriye*, 36, line 4.

⁸⁶ See Kurtoğlu, "Meşhur Türk," 68-73.

combat between Christians and Muslims, came into being by the first quarter of the sixteenth century to give shape to this area of the Muslim Empire: the Mediterranean frontier running from the eastern shore of the Adriatic to Egypt, and the Red Sea frontier. Within these areas the maritime community, whose commerce, shore lines, and peoples provided the reasons for the Ottoman fleet, had its center in the eastern Mediterranean, with the Black Sea, the Persian Gulf, and Red Sea acting as appendages. Algeria, the one province to be added that was significantly outside the sea boundaries of Selim's state, remained very much a part of the forward frontier, the home of the corsair. The axis, then, that gave geographical unity to the Ottoman seaborne empire lay north and south. Along with the Russians, the Safavids, the Mughals, the Venetians, and the Ragusans, the Ottomans had molded imperial and commercial structures that ran perpendicular to the older political and economic organization of the Eurasian steppe. This history, and not the oceanic voyages, moved the locus of imperial activity in Eurasia during the sixteenth century.

In the age of oceanic discoveries the Ottomans and the Portuguese constructed seaborne empires that met in the Indian Ocean. Neither empire built up its naval strength suddenly and, in general, a rough similarity existed in the naval development of the two states. The historical importance, however, of this simultaneity is not that Ottoman imperialism caused Portuguese expansion but that the Indian Ocean contact brought together the most militant representatives of the two cultures. At the turn of the sixteenth century, while Ottoman *ghāzīs* enlarged the boundaries of a state whose growth came from success in the Holy War, a small Christian nation, born in the *Reconquista* on the western Muslim frontier, used its equally small, but technically unique, navy to challenge the Islamic world in a new area. The Ottomans vigorously opposed the Portuguese, reacting to the implications of their naval supremacy near the shores of Arabia, and not off Morocco. The meeting of the Portuguese and the Ottomans off South Arabia, therefore, represented the continuation of old Muslim-Christian conflicts by states that carried on, in the name of religion, the warrior traditions of two different cultures. No new structure embracing Muslims and Christians emerged from this imperial contact. True, the navigations of the Portuguese heralded, centuries later, the technological and economic superiority of the West, but the resistance they met, symbolized in the appearance of a new sea frontier, continued the long-standing cultural differences between Muslims and Christians. Both events are historically significant.

While the Portuguese created a commercial and oceanic empire, the Ottomans pushed their frontiers into the waters surrounding the eastern Mediterranean to create a seaborne state conditioned by the military and administrative requirements of a land-based Turko-Muslim state. In the course of their voyages rulers and merchants from Portugal and other Christian states participated in overseas commercial and military ventures that, in the East, rarely went beyond the establish-

ment of a fortified trading post. While Portugal rejected the conquering tradition of her warrior aristocracy to lean almost entirely upon maritime commerce as the primary reason for imperial naval expansion in the East, the Ottomans, in contrast, sought to conquer territories in order to gain tax revenues from newly acquired agricultural and commercial economies. Thus the conquest system of the Ottomans reflected the desire to administer and tax numerous lands and peoples, for only ample resources could support the vast armies and bureaucracies that ruled the Muslim Empire. By the time both naval empires reached Egypt, the differing economic objectives of the two expanding states stood fully revealed. While Albuquerque had sought to apply a policy aimed at monopolizing the sea trade of the Indian Ocean, Ottoman sultans ordered new registrations of the lands and peoples of Egypt for the purpose of taxation⁸⁷ and, at the same time, directed the outfitting of small fleets to counter the Christian sea blockade.

To measure, then, the success or failure of Ottoman naval efforts in the sixteenth century by the degree of their control over the Indian Ocean spice trade would render a judgment on the Ottoman use of sea power from the economic standpoint of the Portuguese. Ottoman and Portuguese sources generate a broader basis for an assessment. When Ottoman armies reached South Arabia the sultan faced the question whether or not to make a major effort over great distances against strong fleets and under difficult supply conditions to drive the Portuguese sailors from their positions astride the trading routes of the Indian Ocean. His decision to invade Hungary in 1526, and the subsequent commitment of only enough military resources to carry on a frontier war in the Persian Gulf and Red Sea, meant that the economic advantages of large naval operations in the Indian Ocean did not outweigh the gains from expansion in the Balkans, which were more accessible to both the armies and the tax system of the Ottomans. The Portuguese elected, on the other hand, not to occupy more land than that needed for the protection of the distant frontier posts serving their limited commercial interests. Off South Arabia, therefore, the economic structure of each empire encouraged an imperial standoff, for the new sea frontier also marked the limits of effective expansion for an empire whose economic aims were largely agricultural and for a state whose interests were primarily commercial.

To accomplish their aims within ever-widening sea spaces, Ottoman sultans organized the eastern Mediterranean naval experience within their territories and applied it to the military problems brought on by imperial growth along the shores of the Mediterranean and Black Seas. Defeating the Venetians in the years when the Portuguese circumnavigated Africa, the Ottomans went on to use their Mediterranean naval organization in the Indian Ocean against new Christian fleets. Off South Arabia, therefore, the three-masted, broadside Atlantic sailing ships of the Portuguese encountered the Mediterranean galley technology of the Mamluks and the Ottomans.

If, in order to describe the meeting between the two naval practices, the frontier

⁸⁷ Shaw, *Financial and Administrative Organization*, 16-18.

conditions south of Egypt, rather than the outcome of specific battles, are examined, then the seagoing technical abilities of each side can be seen to support the differences between these two seaborne empires. The galleys, the manpower, and the heavy cannons of the Ottomans gave the Muslims sufficient strength to confront the Portuguese in the narrow, steamy waters off Arabia, to expand and defend positions on land, and to protect some portion of the coastal trade. For the Portuguese, the technological advantages of their sailing ships did not yield sufficient power in the Indian Ocean to allow them full control over the frontier conditions in the East; rather their ships supported the erection and defense of widely scattered trading posts and a naval supremacy on the open sea. Thus the Red Sea, and later on the Persian Gulf, became the southern limit of effective Mediterranean galley warfare, while the Indian Ocean became the home of the Atlantic sailing ship. By the end of Selim the Grim's reign in 1520, the two naval technologies took their place beside other forces in a historic division of lands and seas separating the Portuguese and Ottoman empires.⁸⁸

The historiographical boundaries imposed by language and culture, which have clouded the division of two different societies along an Indian Ocean frontier of historic importance, can now be crossed through the comparison of modern Ottoman and Western naval history. Against a background of sixteenth-century Ottoman history the hypothesis of "lost opportunities" faults the Ottomans for not adopting the priorities of quite different Western commercial states in the very age when Muslim institutions proved successful. Only centuries later was it clear that the commercial and technical predominance of the Christians had resulted in the political domination of large Muslim states by small European nations. Meanwhile the sixteenth-century leaders of the Ottoman state faced the naval problems of a Turko-Muslim Empire erected over the debris of older Near Eastern states. By the standards of that century and according to the institutions that formed their society, the Ottomans successfully met the various external naval challenges on their frontiers, as the cohesive growth of a large sixteenth-century seaborne empire demonstrated.

Elaborating on the idea that the Portuguese sea route to India represented a visible and fundamental challenge that the Islamic world failed to meet, Professor Allen sees the military campaigns of Selim the Grim as a Muslim response along interior land lines against the flanking attack of the Western mariners.⁸⁹ As

⁸⁸ The border zone between the two different societies is marked by a series of frontier ports that separate seas with Mediterranean characteristics—the Red Sea and the Persian Gulf—and the coasts of the northern Indian Ocean from the central areas of that same body of water. Thus the ports of Aden, Berbera, and Muscat, the straits at the Bab al-Mandab and Ormuz, as well as the harbors in Gujarat formed a frontier recognized by sixteenth-century historians. For example, De Barros stated that galleys operated better than sailing ships in the Red Sea (*Asia*, decade III, bk. I, chap. II, 14), while Ulughkhānī argued in the case of culture that Aden, as well as Yemen, was on the frontier of the Islamic world (I, 218).

⁸⁹ William E. D. Allen, *Problems of Turkish Power in the Sixteenth Century* (London, 1963), 1-14. Fernand Braudel has a more balanced view: Egypt was conquered for reasons internal to the Mediterranean, but the conquest then embroiled the Ottomans even further in a world war with the Christians. *La Méditerranée et le monde Méditerranéen à l'époque de Philippe II* (2d ed., Paris, 1966), II, 16-19, 170-72.

Ottoman and Mamluk sources indicate, the appearance of the Portuguese in the Indian Ocean threatened the Holy Cities and Muslim trade, causing Muslim rulers substantial concern. But the Ottoman reply to the frontier raids of the Portuguese does not support the argument that the Atlantic sailing ships raised a major threat for the Islamic world; otherwise the Protector of the Holy Cities would not have invaded Hungary. Rather than seeing the invasion of Egypt in 1517 as an outgrowth of European history, the Ottoman evidence emphasizes one of the grand themes of Near Eastern history: since the spread of civilization along the shores of the Mediterranean, the position and wealth of Egypt have attracted imperial adventurers of which the Ottomans were not the last.

Why did the Ottomans, in an age when they mobilized the talents of raw frontiersmen who were imperfectly exposed to the influences of traditional Islam, not adopt the new naval technology of the Portuguese just as they had acquired the firearms of the Europeans in an earlier period? Professor Cipolla has argued that the sociocultural institutions of the Ottoman Empire provided little basis for the adoption of Western technology. This is certainly true. The use of large cannons, numerous men, and unstable galleys for summer naval operations to take land objectives for an army whose main economic support came from agricultural holdings would hardly encourage the type of oceanic naval technology developed by small European commercial states. Ottoman sources, however, also indicate that the success, not the failure, of traditional Mediterranean naval practices influenced Ottoman leaders to continue the use of an "obsolete" technology.

The contours of the Mediterranean and not the open areas of the Indian Ocean were the main boundaries for the sixteenth-century Ottoman navy. If Mediterranean naval history through the first half of that century is examined, Ottoman fleets successfully competed with the galleys of their Christian opponents. Even after the battle of Lepanto in 1571 the Ottomans rebuilt with galleys and went on to consolidate their hold over North Africa by taking the Spanish forts at Tunis in 1574. The difficulty of determining when the naval balance of power tilted against the Ottomans results, to a great degree, from the peripheral contact of the two technologies. In the early sixteenth century the Portuguese sailing ships did not mount a direct assault against the Mediterranean heart of the Ottoman seaborne empire, but operated, instead, on the southern edge of Muslim territories. Taught by English and Dutch privateers, Muslim corsairs on the frontier in North Africa would adopt the naval technology of the Atlantic states by the end of the sixteenth century. But the Ottoman Empire, then beset by internal problems, faced no seaborne challenge that called for a change in its basic naval institutions through the use of a new frontier experience.

In no other area is the contrast in viewpoints between Ottoman chroniclers and Western historians so vivid as in the historical frameworks employed to describe the imperial naval history of the sixteenth century. For the age of Vasco da Gama, Muslim chroniclers, ignoring border defeats here and there, record the growth of a powerful Ottoman navy whose victories in the Black Sea and eastern Mediter-

anean led to the conquest of Egypt and the control of the two Holy Cities, making the Ottomans the dominant political power in the orthodox Muslim world.

The interpretations of Western historians, on the other hand do not reflect this triumphant creation of another major Eastern empire. Since the rise of the West followed the decline of the Ottoman Empire, Western historians naturally concentrated their attention on those developments that both limited Muslim expansion and marked the arrival of a new era. Even though the peripheral character of the Ottoman-Portuguese naval contact is acknowledged in some studies, the circumnavigation of Africa, the Indian Ocean naval wars, the *Reconquista* in Iberia, and other Muslim difficulties in Europe form, in the Western view, a chain of frontier failures—even the battle of Chaldiran is a failure in the argument of Professor McNeill⁹⁰—that mark out the boundaries of Ottoman expansion. In the golden age of Ottoman history, therefore, the conquests of the Ottomans, as seen from the Western side, led to failures and ultimately to the decline of the Muslim world.

When the sources of each culture are unearthed for the age of Vasco da Gama, Portuguese and Ottoman records document the creation of a new oceanic frontier that strengthened, in major proportions, the differences between the Islamic world and the West. Unknown to the Ottomans, the long-range naval expansion of the Portuguese was the outward sign of deep internal changes in Western society that would, centuries later, allow European armies and navies to cross all the frontiers of the House of Islam. Powerful sixteenth-century Ottoman conquests, on the other hand, revitalized a traditional orthodox Muslim community through the creation of a seaborne empire that added to the glory of a great Islamic state.

Accordingly, the historians of the two cultures set down these separate experiences. While Western authors preserved the epochal accomplishments of Portuguese navigators who, immortalized in the poetry of Camões, sailed *por mares nunca dantes navegados*,⁹¹ Muslim historians, writing about the same centuries, had ample material for histories that would maintain the unity of Islamic society.⁹² Certainly the populations along the shores of the Mediterranean, the Black Sea, the Persian Gulf, the Red Sea, and the northwest portion of the Indian Ocean who saw the banners of sixteenth-century Ottoman fleets unfurl before them had no feeling that in 1500 the Ottomans had suffered a major setback from which they would never recover; rather the galleys of the Turks confirmed for both Christians and Muslims the imperial title of the sultan—Shadow of God on Earth.

⁹⁰ McNeill, *Rise of the West*, 621.

⁹¹ Luíz de Camões, *Os Lusíadas*, in *Obras Completas*, ed. Hernâni Cidade (Lisbon, 1962-68), IV, 1 (canto I, line 3). The English translation by Richard Fanshawe is "Through Seas which never Ship had sayld before." See Richard Fanshawe, trans., *The Lusiad* by Luíz de Camões, ed. J. D. M. Ford (Cambridge, 1940), 29.

⁹² For the large number of manuscripts celebrating sixteenth-century Ottoman conquests, consult Ağâh Sırrı Levend, *Gazavât-Nâmeler ve Mihaloğlu Ali Bey'in Gazavât-nâmesi* (Holy War Accounts and the Holy War Account of Mihaloğlu Ali Bey) (Ankara, 1956), 19-102. Finally Marshall G. S. Hodgson, in "The Role of Islam in World History," also challenges the Western assumption of Muslim decline after the year 1500. *International Journal of Middle East Studies*, 1 (1970), 99-123.

World War I and the Liberal Pacifist in the United States

CHARLES CHATFIELD

THE meaning of the word pacifist changed under the pressure for patriotic conformity in 1917-18. Having had the benign connotation of one who advocated international cooperation for peace, it narrowed to mean one who would not support even a "war to end war." Pacifists were linked with draft dodgers, socialists, and communists, portrayed in hues from yellow to red; a rude inscription in the lobby of 70 Fifth Avenue in New York, where some pacifist groups were housed, read, "Treason's Twilight Zone."¹ The word was thenceforth plagued with double meaning, and more than one prowar peace advocate hastened to explain that "those who are now called 'pacifists' here do not include all or most of those who were called 'pacifists' before the war."² Later, when it was respectable to be against the war, the word sometimes was used in its original, broader sense, but it would be used by pacifists themselves, as it is used here, to designate those who worked for peace and refused to sanction any given war—absolute and religious but also selective and political objectors.³ The narrowness of this definition masked a new dimension in the American peace movement.

There had been pacifists in the strict sense before World War I, but for the most part they had been sectarians motivated by obedience to religious injunctions against killing and against complying with the military. Their churches supplied most of the conscientious objectors in the Civil War and both world wars, but these were nonresistants obedient to the claims of religious faith and not challenging governmental authority or social policy except in the specific cases of their military service.⁴

► Mr. Chatfield, associate professor of history at Wittenberg University, where he specializes in the social and intellectual history of the United States, is particularly interested in the organized peace movement. His book on the pacifists and the peace movement in the United States, 1914-41, is scheduled for publication in 1971. Mr. Chatfield studied under Henry Lee Swint at Vanderbilt University, receiving the doctorate in 1965.

¹ This graffito is mentioned in "Memoir of Frances Witherspoon and Tracy Mygatt," p. 8, Oral History Collection, Columbia University. Both women were active in the peace movement and especially the civil liberties groups in New York during World War I.

² Julia Grace Wales to Clark F. Hunn, Nov. 28, 1917, Wales Papers, Wisconsin Historical Society, Madison, Wisconsin (italics in the original are omitted). Julia Wales was an instructor in English at the University of Wisconsin and the author of a plan for continuous mediation by an international commission of citizen experts.

³ Pacifism is sometimes defined still more narrowly as the position of only those who are opposed to all war. This is the basis for legal recognition of conscientious objection in the United States, and there is considerable merit in its usage. Philosophically and politically speaking, this is a tidy definition, but it is not historically useful since the impact and significance of pacifism varied with its changing constituency and since pacifists of all persuasions responded to the same historical events.

⁴ The peace churches are traditionally designated as the Quaker, Mennonite, and Brethren, but

If sectarians had eschewed social reform, few progressives had stressed the war question, and even fewer seriously considered conscientious objection to war service. Indeed, the prewar advocates of peace hardly sensed the possibility of divided loyalties. They assumed that war was anachronistic. Reason, embodied in arbitration and law, in treaties and international juridical institutions such as the Hague Court, would obviate recourse to war. In this respect peace advocates were internationalists, and so they liked to think of themselves. But with few exceptions they were solid nationalists as well, for they assumed that America's virtues were unique and her interests paramount. If the nation ever should go to war, they believed, its democratic politics and humanitarian traditions would guarantee its cause to be just and necessary.

These peace advocates either were directors of business and educational institutions or accepted such men as models. They gave their movement a literary, patriarchal, and elitist quality, and relied on education and discussion rather than political action. They operated the Carnegie Endowment for International Peace (founded in 1910), the World Peace Foundation (1910), the Church Peace Union (1914), the American School Peace League (1908), the American Peace Society (1828), and various other groups organized to promote study, friendship, and arbitration. They were established men who valued order and distrusted radical challenge to authority, successful men who assumed that progress was inevitable and who aimed at the further perfection of society.⁵

Sarajevo shattered the doctrine of perfection, at least as it applied to Europe. The established peace movement faltered and fell into disarray; by April 1917 most of its leaders had joined the war effort, determined to establish a universal peace along American lines. Accustomed to look for evil on the surface, not in the heart of man, they identified it with one nation—Germany. Peace was held at bay by Prussianism, they said; victory became the prerequisite of progress.⁶

Those who rejected this view and advanced alternatives to it were the wartime

others important for pacifism include the Disciples of Christ and Jehovah's Witnesses. The definitive study of religious pacifism is Peter Brock, *Pacifism in the United States: From the Colonial Era to the First World War* (Princeton, 1968). See also Roland Bainton, *Christian Attitudes Toward War and Peace: A Historical Survey and Critical Re-evaluation* (Nashville, 1960), and, regarding the Quakers, Elbert Russell, *The History of Quakerism* (New York, 1942), Rufus M. Jones, *The Later Periods of Quakerism* (London, 1921) and *A Service of Love in War Time: American Friends Relief Work in Europe, 1917-1919* (New York, 1920), and Lester Jones, *Quakers in Action* (New York, 1929).

⁵ The prewar peace movement is studied thoroughly in David S. Patterson, "The Travail of the American Peace Movement, 1887-1914" (Ph.D. dissertation, University of California, Berkeley, 1968), and interpreted in Charles R. Marchand, "The Ultimate Reform: World Peace in American Thought During the Progressive Era" (Ph.D. dissertation, Stanford University, 1964). Varying strands of the movement's rationale are developed in Sondra R. Herman, *Eleven Against War: Studies in American Internationalist Thought, 1898-1921* (Stanford, 1969). The older and established histories of this movement are, of course, Merle Curti, *Peace or War: the American Struggle 1636-1936* (New York, 1936), and Devere Allen, *The Fight for Peace* (New York, 1930).

⁶ This argument is oversimplified in comparison with its most restrained and balanced presentation (Harry Emerson Fosdick, *The Challenge of the Present Crisis* [New York, 1917]), but it is underplayed in comparison with many contemporary slogans—even those of religious and peace organizations. See Ray H. Abrams, *Preachers Present Arms* (New York, 1933), and Horace C. Peterson and Gilbert Fite, *Opponents of War, 1917-1918* (Madison, 1957).

pacifists. They reorganized the American peace movement, giving it much of the structure, leadership, social concern, and rationale that would characterize it for over a generation. Where it had been educational and legalistic, the peace movement became political as well; where it had been polite it also became aggressive; where it had been conservatively Brahmin, it also acquired a socialist base; where it had assumed progress, it would claim only possibility. The movement remained divided—perpetually, it seems—between competing points of view and programs, but the wartime pacifists gave it vital leadership and broad social concern.

They brought to it an unresolved dilemma, too, for their experience imparted both a more radical view of society and an ethic of conflict that proscribed the use of violence for social change. No less committed to liberal values than were the intellectuals who stoutly defended the war, pacifists interpreted it differently. No less fervent in their opinions than the patriots, they were virtually isolated from public opinion. When they were subjected to social pressure to conform, pacifists came to distrust authoritarianism itself and to connect it in their minds with violence. That is one reason why their leftward movement stopped short of revolutionary socialism. Associating injustice with war, they hobbled the drive for social justice with a commitment to peace.

Liberal pacifists were the remnant of a peace coalition composed largely of progressives who viewed the war as a threat to the values for which they had worked. War must not come to America, they agreed; and, moreover, its very existence in Europe challenged that notion of an open-ended world of social possibility in which these problem-solvers believed. This war was no abstraction. It was a compelling problem, they insisted, and its solution required concerted social action. This was the response of Louis Lochner who, with the help of George Nasmyth and the American Peace Society, had organized the Cosmopolitan Club movement in American universities, the man who before 1914 had personified internationalism to thousands of college students. After the outbreak of war he left the American Peace Society, tried to refashion the Chicago Peace Society, and together with Jane Addams and others launched a National Peace Federation.⁷ It was one of several new organizations federating liberal and peace forces that emerged from such centers of social reform as the Henry Street settlement house founded in New York by Lillian Wald. There social workers, clergymen, educators, and publicists who were conscious of a bond of social concern they had formed in response to industrialism and urbanism met in response to war.⁸

⁷ Other organizations in which Lochner played a leading role included the Ford Peace Expedition (November–December 1915), the First American Conference for Democracy and the Terms of Peace (May 30–31, 1917), and the People's Council of America (May 30–31, 1917). He was also active in other peace movements of the period.

⁸ Groups whose primary impetus came from New York included the American Union Against Militarism (April 1916), the American Neutral Conference Committee (July 1916), the Emergency Peace Federation (February 1917), the People's Council, and the National Civil Liberties Bureau (a

When the Woman's Peace Party was founded in Washington on January 10, 1915, it was clear that advocacy of peace "provided a common ground upon which could meet American women from almost every important section of their organizational life."⁹ The women quickly joined hands with their counterparts in Europe; together they developed a plan for a conference of neutral nations that would stand ready to clarify the war aims of belligerents and to negotiate peace. They sent emissaries to the belligerent leaders and tried to induce President Wilson to adopt their program. He seemed unresponsive, and so their diplomacy evolved into a commission of private citizens to which Henry Ford gave funds and publicity. Ridicule of Ford's Peace Expedition obscured the serious purpose of a significant nongovernmental international organization. Still, the women who promoted it had forged organizational links with civic and professional groups, connections that would survive the war. They brought new leadership into the peace movement and created a modern pressure group of a kind familiar to progressive reformers.

Meanwhile, some Quakers, social gospel clergymen, and YMCA leaders responded to the organization of religious pacifists in England when, on November 11, 1915, they created the Fellowship of Reconciliation (FOR), which became the central body for religious objectors for the next half century.¹⁰ Moreover, liberal journals such as the *Survey*, the *Independent*, and the *Nation* opened their pages to proposals for a neutral conference for peace in Europe and to arguments against preparations for war at home. Indeed, by the time the Ford project became a laughingstock, peace workers from Henry Street were bringing the progressive peace coalition to its culmination in the American Union Against Militarism.¹¹ Historically important for its large-scale antipreparedness campaign

separate organization as of October 1, 1917). The Woman's Peace Party (established January 10, 1917) and the Fellowship of Reconciliation (November 11, 1915), although New York oriented, did not stem primarily from the Henry Street group but included many of its members.

⁹ Marie Louise Degen, *The History of the Woman's Peace Party* (Baltimore, 1939), 40. For the history of the successor to the WPP, see Gertrude Bussey and Margaret Tims, *Women's International League for Peace and Freedom: 1915-1965* (London, 1965). The story is told through biography in James Weber Linn, *Jane Addams, a Biography* (New York, 1937), John C. Farrell, *Beloved Lady: A History of Jane Addams' Ideas on Reform and Peace* (Baltimore, 1967), Mercedes M. Randall, *Improper Bostonian: Emily Greene Balch* (New York, 1964), Jane Addams, *Peace and Bread in Time of War* (New York, 1945) and *Second Twenty Years* (New York, 1930), and Louis Lochner, *Henry Ford—America's Don Quixote* (New York, 1925). Regarding the role of Julia Wales, see William Trattner, "Julia Grace Wales and the Wisconsin Plan for Peace," *Wisconsin Magazine of History*, XLIV (1961), 203-61.

¹⁰ Membership in the FOR involved signing a declaration of principles. Consequently its membership rolls are the best index of pacifist intention for peace advocates, and its minutes and publications are the best sources on the rationale of religious pacifism. They are collected in the Swarthmore College Peace Collection (hereafter SCPC), but since much from the war period is missing, they must be supplemented with the papers of Gilbert Beaver, Norman Thomas (New York Public Library, hereafter NYPL), John Nevin Sayre (personal possession, Nyack, N.Y.), and others.

¹¹ The Anti-Preparedness Committee, established in November 1915, became the American Union Against Militarism (AUAM) on April 3-4, 1916. Only the name changed in the context of a determined antipreparedness drive from April 6 to May 6; the leadership and rationale of the organization remained the same. The best published account of the AUAM is in Donald Johnson, *The Challenge to American Freedom: World War I and the Rise of the American Civil Liberties Union* (Lexington, 1963), but see also Robert L. Duffus, *Lillian Wald: Neighbor and Crusader* (New York, 1938), John Haynes Holmes, *I Speak for Myself* (New York, 1959), Rabbi Stephen S. Wise, *Chal-*

of April and May 1916 and for its role in preventing a full-scale war with Mexico in June, the American Union became a model for postwar peace lobbies, and from its ranks emerged both the Foreign Policy Association and the National Civil Liberties Bureau. Its leaders included many of those active in domestic reform and Progressive politics who feared that militant nationalism would sap social progress and frustrate open diplomacy and world federation. Throughout 1916 they supported Wilson with alternating reluctance and enthusiasm as he seemed to act against or speak for their principles.

In 1917 three events shook the progressive peace coalition and reduced it to pacifism. First, when Wilson severed relations with Germany in February, the American Union lost one of its most ardent members, the influential rabbi of New York's Free Synagogue, Stephen Wise. Antiwar leaders who had valued his example included Paul Kellogg, editor of the *Survey*, and Emily Balch, founder of the Denison House settlement in Boston and active in the Woman's Peace Party. They were stunned, but nonetheless participated heartily in a new Emergency Peace Federation to keep America out of war. Throughout the spring pacifists enlisted much popular support, but when in April the United States entered the war, prominent peace leaders, including David Starr Jordan, the chancellor of Stanford University, left their ranks.¹² Even so, they were encouraged by the fact that six senators and fifty representatives voted against the war resolution and by the opposition voiced by the Socialist party after the resolution passed. There was still some basis for hoping that they might influence public policy, and so pacifists created the People's Council of America for Peace and Democracy in order to advance civil liberties and democratic peace terms during the war. By September 1917, however, the Council had aroused so much public opposition by criticizing conscription and defending the Russian Revolution that it did not seem useful to Lillian Wald, Paul Kellogg, and some others who had initiated the new peace coalition.

The American Union Against Militarism was divided during the summer by the efforts of some of its leaders to commit it to the cause of conscientious objectors. Three pacifists were particularly active: Roger Baldwin, who came to the staff from a position as secretary of the Civic League of St. Louis and who shortly organized the Civil Liberties Bureau; Norman Thomas, who was a socially concerned Presbyterian minister and who later joined the Socialist party because of his pacifist beliefs; and vivacious Crystal Eastman, an expert

lenging Years (New York, 1949), Michael Wreszin, *Oswald Garrison Villard: Pacifist at War* (Bloomington, 1965), and David Starr Jordan, *Days of a Man* (New York, 1922), II, 690-707, 712-36.

¹² *Ibid.*, 734-36. Regarding Wise's change of view see Carl Herman Voss, *Rabbi and Minister: The Friendship of Stephen S. Wise and John Haynes Holmes* (New York, 1964), 141-43.

The Emergency Peace Federation was formed out of the American Committee for a Neutral Conference, but its leadership overlapped with the AUAM. Lillian Wald and Paul Kellogg helped to found a "committee on Nothing at All" in April 1918, which had the nucleus of the original AUAM and which evolved into the Foreign Policy Association. See Lillian Wald, *Windows on Henry Street* (Boston, 1934), 311.

on the legal aspects of industrial accidents who was active in the women's suffrage campaign and the New York branch of the Woman's Peace Party and who with her brother Max later edited the antiwar *Liberator*. The membership of the American Union was at no time entirely pacifist in the strict sense, and these driving leaders threatened to undercut its constituent base and respectability.

On August 20, in the absence of Miss Wald, the executive board of the American Union voted to send delegates to the People's Constituent Assembly of the People's Council of America. She eventually resigned from the board; others followed, and the Union was shattered. It continued to exist in nominal fashion, but the National Civil Liberties Bureau separated itself on October 1, 1917, and thereafter the American Union Against Militarism operated largely on paper only.¹³ During the war liberal pacifists affiliated with several other groups that have lasted over half a century: the Civil Liberties Bureau, the Fellowship of Reconciliation, the Woman's Peace Party and its successor, the Women's International League for Peace and Freedom, and the American Friends Service Committee (founded in 1917 to provide humanitarian alternatives to fighting).

In their opposition to the war the remnant of the progressive peace coalition was linked with literary radicals (including most of the staff of *The Masses*) and with those Socialists who supported the antiwar resolution that their party made in St. Louis on April 7, 1917. The party convention had been largely middle class in composition, and although its majority report was cast in the language of anticapitalism, it advocated a platform like those of the peace groups rather than a program of revolution or general strike.

These opponents of war were joined by new recruits who in the long run were most important of all, since they virtually staffed the pacifist movement after peace was re-established. They included, among others: A. J. Muste, a Congregational minister who subsequently became a leader in the labor and Trotskyite movements, chairman of the Fellowship of Reconciliation, and the symbol of radical pacifism to a cold-war generation; John Nevin Sayre, an Episcopal minister who was never far from the center of the International Fellowship of Reconciliation or its American branch; Evan Thomas, an outstanding conscientious objector of World War I and chairman of the War Resisters League during the Second World War; Kirby Page, a YMCA worker who became the most influential pacifist speaker and writer of the interwar period; Ray Newton, active in Quaker relief work, who later directed the Peace Section of the American Friends Service Committee; Frederick Libby, Florence Boeckel, and Dorothy Detzer, who operated an influential peace lobby in Washington during the thirties; Devere Allen, a student at Oberlin College who became the chief advocate of war resistance in the Socialist party; and the subsequent leader of that party, Norman Thomas.

Few of those converted to pacifism during the war had been active in peace

¹³ Minutes of Aug. 30, Sept. 13, and Oct. 1, 1917, and *passim*, AUAM Papers, SCPC.

groups before 1917, and they thought through the war question by themselves. They had not been active in domestic reforms, but they were, indeed, just discovering social problems—some through college experiences, some through church work, and others in the fresh idealism of the YMCA, then promoting international concern through the Student Volunteer Movement. In short, the young pacifists encountered World War I when they were coming of age socially, just as progressives of a previous generation had awakened to contemporary problems when they were choosing personal directions. It is hardly surprising that many of those whose pacifism commenced in the war years made peace work a vocation.

There were liberal pacifists of various hues, then, and their language and experiences differed significantly. The very corollary of conscience is, in the apt phrase of Rufus Jones, “a final farewell to uniformity,” so that any analysis of the movement is hazardous.¹⁴ Moreover, pacifists’ ideas appear more coherent in retrospect than they did when first published, because ideas that we now analyze in terms of common postulates were first advanced polemically by persons whose lives were strikingly dissimilar. This is not to suggest that pacifists acted altogether rationally. Rather, they were drawn together in action through their similar interpretations of their various experiences. If the history of ideas is the story of men’s reflections upon their experiences, then it is the study of what meaning they assigned to life; and meaning, if not life, has logical form. What pacifists had in common that set them apart from war supporters was neither a covert conspiracy nor any discernible personality or set of social characteristics. They shared, instead, a distinctive view of the war and a disposition to elevate that view into a matter of principle.

To begin with, pacifists accepted such liberal values of progressivism as the pragmatic approach to choices, the democratic process, and the ultimate worth of the individual. These were hardly more than loosely defined notions, but they implied at least the following: that decisions should be made in the light of consequences rather than of a priori rules and that the meaning of social institutions is found by experiencing them; that political power should be distributed in a society in which economic power is highly concentrated and that decision making should be broadly based; and that individuals are the ends for whom society is ordered. Sentiments like these can be found in a wide variety of objections to the war, even among the disparate arguments of socialists.

The leaders of the Fellowship of Reconciliation put their notion of human value in religious terms, agreeing with Norman Thomas that war is “absolutely opposed to Christ’s way of love and His reverence for personality.”¹⁵ Although they did not define personality, they often referred to it in the sense of a man’s

¹⁴ Jones, *A Service of Love*, 105.

¹⁵ Thomas, “Some Objections Considered,” in *The Conquest of War: Some Studies in a Search for a Christian World Order*, ed. Norman Thomas (New York, 1917).

total being and latent possibility. They rejected the notion of prowar clergymen that combat or even death could leave men undefiled, could even ennoble them. War immolates personalities, they said. In the oldest tradition of their faith, religious pacifists revolted "not only against the cruelty and barbarity of war, but even more against the reversal of human relationships which war implied."¹⁶ The doctrines of love, fatherhood, and brotherhood and such symbols as the cross expressed the normative value of personality for pacifists. Their rhetoric would sound formal to a later generation, but to them it expressed a long-neglected doctrine of Christian faith, the fundamental worth of each individual in the sight of God.

If human personality was sacred to religious pacifists, it had nearly absolute value for some who stood on secular grounds as well. Roger Baldwin, the director of the National Civil Liberties Bureau, spoke for them. On trial for refusing to take the physical examination required in the draft, he said, "The compelling motive for refusing to comply with the Draft Act is my uncompromising opposition to the principle of conscription of life by the state for any purpose whatever, in time of war or peace."¹⁷ At the same time, he felt an intense social concern. As Norman Thomas explained shortly after the war, the individual is a "product of the group, but the group is only valuable as it permits personalities, not automata to emerge."¹⁸ Liberal pacifists, unlike Spencerian individualists, supported social reform, but, unlike those whose individualism derived from concepts of natural law, they believed that every man is of intrinsic value. Some of them reconciled individualism and socialism, for example, by assuming that man is essentially a social animal and that an individual's personality is most fully realized in altruistic impulses.

The sense of the individual was muted for most socialists by awareness of class, but even so they argued that the organized proletariat itself was "proclaiming the glad tidings of the coming emancipation," freedom from the tyranny of class over men.¹⁹ Socialists were most strongly united against military conscription. A few supported it, to be sure. William English Walling accused his opponents of accepting conscription by foreign governments that they favored while "leaving America helpless."²⁰ On the contrary, most socialists who op-

¹⁶ Jane Addams, *Peace and Bread*, 4. See also W. Fearon Halliday, *Personality and War* (New York, 1916).

¹⁷ "Statement in Court," Oct. 30, 1918, enclosed in a letter from Norman Thomas to Harry W. L. Dana, Oct. 31, 1918, Dana Papers, SCPC. It was subsequently printed as *The Individual and the State: the Problem as Presented by the Sentencing of Roger N. Baldwin* (New York, 1918). For other examples of this position see Ernest L. Meyer, "HEY! YELLOWBACK!": *The War Diary of a Conscientious Objector* (New York, 1930), and the discussion of nonreligious objection in Clarence M. Case, *Non-Violent Coercion: A Study in Methods of Social Pressure* (New York, 1923), 251-64.

¹⁸ Thomas, *The Conscientious Objector in America* (New York, 1923), 29.

¹⁹ *Voices of Revolt: Speeches of Eugene V. Debs*, introd. Alexander Trachtenberg (New York, 1928), 74.

²⁰ "Socialists and the Problems of War: A Symposium," *Intercollegiate Socialist*, V (1917), 26. Socialist views on the war are interpreted in their diversity in James Weinstein, *The Decline of Socialism in America, 1912-1925* (New York, 1967), but see also Peterson and Fite, *Opponents of War*, and Morris Hillquit, *Loose Leaves from a Busy Life* (New York, 1934). The best sources for

posed conscription believed that "it robs the individual of freedom" and is "the readiest tool of the military class."²¹ As Emily Balch wrote, "It means conscription of mind, hierarchical stratification of society, industrial discipline on [a] military model, obedience as the prime virtue."²² Even those socialists who could support a war to save the country from militarism could not conceive of "militarism to save us from war."²³ The commitment to individual worth and freedom permeated the publications of pacifists of all political views. For many this commitment was in itself sufficient reason to refuse military service; for all it guaranteed the right of conscientious objection.

Objection to military service was interpreted as the right to dissent by many pacifists who regarded that right as a corollary to the democratic process of majority decision. Like abolitionists before them, the pacifists won support on civil liberties that they could not get on the war issue.

The American Union Against Militarism had come into being largely in the vague apprehension that preparedness, conscription, and war would undermine the gains of the Progressive era.²⁴ Its programs and techniques expressed the progressive faith in the power of public opinion and in government responsible to the people. Woodrow Wilson expressed the same political faith even as he pressed for policies that distressed the pacifists. Even in February 1917 most members of the American Union's executive board preferred to leave foreign policy in Wilson's hands. Increasing numbers of pacifists became apprehensive as the administration geared up for war, although Crystal Eastman wrote in June of the president's wartime appointments:

It [is] as though he said to his old friends, the liberals, "I know you are disappointed in me—you don't understand my conversion to the draft—my demand for censorship. I have reasons, plans, intentions, that I can't tell you. But as guarantee of good faith I give you Baker and Keppel and Lippman and Creel, to carry out these laws. No matter how they look on paper, they cannot be Prussian in effect with such men to administer them."²⁵

the period are pamphlet literature, notably *The American Socialists and the War*, ed. Alexander Trachtenberg (New York, 1917), and periodicals such as *The Masses*, *New Review*, and *Intercollegiate Socialist*. The latter is particularly useful as it printed reasoned arguments representative of both sides as a matter of editorial policy (see statement of May 7, 1917, Intercollegiate Socialist Society Papers, Taminment Institute, New York). The Socialist Party Collection of manuscript sources at Duke University is not strong for this period.

²¹ William E. Bohn and Randolph Bourne in "Socialists and the Problems of War," *Intercollegiate Socialist*, V (1917), 10.

²² *Ibid.*, 9.

²³ Joseph D. Cannon, *ibid.*, 12. Cannon wrote that he could conceive of a legitimate war of national defense.

²⁴ The theme of war as a threat to progressive gains pervades antipreparedness literature. Typical are the following: "Around the Circle Against Militarism," *Survey*, XXXVI (1916), 95; John Haynes Holmes, "War and the Social Movement," *ibid.*, XXXII (1914), 629-30; and Oswald G. Villard, "Shall We Arm for Peace?" *ibid.*, XXXV (1915), 299.

²⁵ Crystal Eastman to members of the executive committee, June 14, 1917, AUAM Papers, SCPC. Newton D. Baker, mayor of Cleveland (1912-16), was appointed secretary of war on March 7, 1916; Frederick Keppel, dean of the College of Columbia University, became third assistant secretary of war; Walter Lippmann, liberal commentator for the *New Republic*, was assistant to the secretary of war, June to October 1917; and progressive newspaper editor George Creel became chairman of the Committee on Public Information on April 14, 1917.

The guarantee was not sufficient. The National Civil Liberties Bureau and related organizations expanded their work rapidly, insisting that the civil rights of conscientious objectors to the war were linked to the democratic process itself; majority decision that rested on the suppression of minorities would be a thinly veiled tyranny. This was exactly the premise of those who wanted to keep the Bureau within the American Union in the fall of 1917. As Norman Thomas said, no other national group was prepared to fight for the "tolerance of minority ideas" that "is absolutely necessary for reasonable social progress."²⁶ Blatant persecution of dissenters aroused in Eugene Debs the fighting qualities that had been depressed by his sensitivity to the tragedy and anguish of war. His devotion to the workers' cause had never lagged, but his anger was rekindled by the flagrant denial of "the constitutional right of free speech in a country fighting to make democracy safe in the world."²⁷ He had never ceased to condemn the war, but he stepped onto the platform again on behalf of socialists' freedom. So persistent was the value of democracy in his mind that in 1920 he denied that the Bolsheviks had really intended a dictatorship, even of the proletariat. For Debs "freedom and equal rights" were inseparable.²⁸

Similarly, pacifists in the People's Council of America who supported the Russian Revolution during the war regarded it as a vindication of the democratic process and not of the Bolshevik party or even, on the whole, of Marxist economics. As Max Eastman wrote, "what makes us rub our eyes at Russia . . . is the way *our own theories* are proving true."²⁹ These pacifists supported the revolution, too, because its peace planks accorded with their own demand for a "new diplomacy" embodying democratic principles such as freedom of press, petition, and speech, a progressive tax on war profits, and a "referendum on questions of war and peace."³⁰

However impractical a referendum on war might appear to be (it proved no more plausible in 1917 than it would twenty years later as the Ludlow Amendment), most liberal pacifists were responsive to the pervasive currents of pragmatism. Norman Thomas heeded them at Union Seminary, Randolph Bourne at Columbia, Kirby Page at Drake and Chicago; but, in fact, pragmatism was construed to support opposing positions on the war. John Dewey and liberals aligned with the *New Republic* (like some prowar socialists) argued that since war prevailed, the intelligent thing to do was to participate so as to be present at that "plastic juncture" when history is being made—the peace settlement.³¹

²⁶ Edward Evans to Crystal Eastman, Sept. 28, 1917, and Norman Thomas to Crystal Eastman, Sept. 27, 1917, AUAM Papers, SCPC.

²⁷ Quoted in Ray Ginger, *The Bending Cross: A Biography of Eugene Victor Debs* (New Brunswick, 1949), 356.

²⁸ *Speeches of Eugene V. Debs*, 55–56.

²⁹ Eastman, *Love and Revolution: My Journey Through an Epoch* (New York, 1964), 45.

³⁰ "Resolutions of the First American Conference for Democracy and Terms of Peace," May 30–31, New York City, Organizing Committee, People's Council of America Papers, SCPC.

³¹ Regarding the *New Republic* group see Charles Forcey, *Crossroads of Liberalism* (New York, 1961), chaps. 7 and 8, and Christopher Lasch, *The New Radicalism in America, 1889–1963: The Intellectual as a Social Type* (New York, 1965), chap. 6.

Randolph Bourne called this a rationalization of intellectual default. By the time of his death in 1918 Bourne's ideas were as familiar to liberal pacifists as was the sight of his hunched back and tortured features. He distrusted religious moralism less only than complacent liberalism, but he came to conclusions similar to those of Christian pacifists, breaking with many of the assumptions and friendships of his past in order to do so. He tried to reach back beyond Dewey's instrumentalism—now a lever for preserving the old order, he thought—to the spirit of William James.

In a world "where irony is dead" he scored prowar intellectuals for their credulity. He was offended as much by the quality of their thought as by their conclusions. "The ex-humanitarian, turned realist, sneers at the snobbish neutrality, colossal conceit, crooked thinking, dazed sensibilities, of those who are still unable to find any balm of consolation for this war," he observed bitterly. The so-called pragmatists had idealized the instruments of policy, he wrote; they had forgotten that "the real enemy is War rather than imperial Germany."³² Did the realists think that they could control events by joining forces already in motion? Perhaps. But a more consistent pragmatism would be less sanguine: ". . . if it is a question of controlling war, it is difficult to see how the child on the back of a mad elephant is to be any more effective in stopping the beast than is the child who tries to stop him from the ground."³³ The tendency to judge things in terms of results typified all liberals. Bourne was atypical only because he was pessimistic about the consequences of national war.

A number of those who would become professional pacifists between the wars had been impressed in college by the developing field of sociology and the prospect of "discovering concrete ways of getting ideals incarnated in actual institutions."³⁴ Their disposition to value pragmatic criteria in decision making set them apart from sectarian nonresistants of the past. Kirby Page, working out his position while helping German prisoners of war through the English YMCA, argued that war had to be judged by what it does:

War is not an ideal, it has an ideal; war is not a spirit, it is waged in a certain spirit; war is not a result, it produces results. War is always and everywhere a *method*, and it is as a method that it must be discussed.³⁵

He concluded that it was unchristian, and so his judgment was perhaps not political, but his approach laid the foundation for empirical analysis of international affairs in the postwar years, if not for selective objection to military service. His friend Evan Thomas—Norman's brother—wrote that "on purely sociological grounds I would oppose the war."³⁶

³² Randolph S. Bourne, "War and the Intellectuals," in *War and the Intellectuals: Essays by Randolph S. Bourne, 1915-1919*, ed. Carl Resek (New York, 1964), 10. A fuller range of Bourne's thought is suggested in Lillian Schlissel, *The World of Randolph Bourne* (New York, 1965).

³³ Bourne, *War and the Intellectuals*, 12.

³⁴ *Ibid.*, 10.

³⁵ Kirby Page to Howard E. Sweet, Feb. 3, 1918, and especially the manuscript, "The Sword or the Cross," Page Papers, Southern California School of Theology, Claremont.

³⁶ Evan to Norman Thomas, Nov. 5, 1916, Norman Thomas Papers, NYPL. This sentiment was an integral part of Evan Thomas' agonizing re-evaluation of religion.

These values—pragmatism, democracy, and the sanctity of individual life—were shared in some degree by all liberals and many socialists. Pacifists universalized them. They applied them even to national policy, even in wartime. They made them “axioms of emotional nature” that lent special force to their distinctive view of the politics of the war.

Liberal pacifists concluded that World War I was a product of the European state system and that American national interests were best served by staying out. They identified the causes of the war in European rivalries, in long-standing “misunderstanding, suspicion, fear, diplomatic and commercial struggle to which all nations contributed.”³⁷ All elements of later revisionist writing on the war question can be found in the antiwar literature of 1917–18. Pacifists and antiwar socialists alike stressed the role of commercial competition, imperialism, secret treaties, and war profits in producing international conflict.

Socialists found in the economic origins of the war clues to its class basis. The workers were as expendable in wartime as they had been in peace, and for the same selfish ends, it was said: “Wars bring wealth and power to the ruling classes, and suffering, death, and demoralization to the workers.”³⁸ At the very least, fighting abroad would “neutralize the class struggle,” as some socialists explained.³⁹ Everything they believed about the war’s origins confirmed their view that it was an imperialistic conflict and “not the concern of the workers.” Moreover, such leaders as Morris Hillquit and Eugene Debs sensed the power of nationalism with its psychological extensions of fear and pride even in the arguments of those socialists who supported the crusade. Hillquit later ascribed the “stifling terrorism” of a “morbid war psychology” to the circumstance in which the major political parties were rivals in promoting the war effort.⁴⁰ Several socialists distrusted the idea of holding a referendum on war precisely because they feared that popular agitation would increase jingoism. Their sensitivity to the power of militant nationalism drew these socialists close to less class-conscious pacifists.

Whereas socialists had a handbook in George Ross Kirkpatrick’s unbridled Marxist indictment, *War, What For?*, liberal pacifists found their thinking reflected in Norman Angel’s analysis of the fallacy of viewing national defense as security, *The Great Illusion*.⁴¹ Conflict of economic interest was the under-

³⁷ John Haynes Holmes, *The International Mind* (New York, 1916), 7, but see his full argument in *New Wars for Old* (New York, 1916) and wartime pamphlets, as well as in other pacifist literature, including especially the FOR journal, *The World Tomorrow*, edited by Norman Thomas.

³⁸ The majority report of the Socialist party, adopted in St. Louis on April 11, 1917, is printed in full in Nathan Fine, *Labor and Farmer Parties in the United States, 1828–1928* (New York, 1928), 310–14. The report was written by Morris Hillquit and Charles Rutherford, among others. A minority report by Louis Boudin is also printed in full in *ibid.*, 315–17.

³⁹ Alexander Trachtenberg “Socialists and the Problems of War,” *Intercollegiate Socialist*, V (1917), 25.

⁴⁰ Hillquit, *Loose Leaves from a Busy Life*, 169.

⁴¹ About 150,000 copies of *War, What For?* were sold between its publication by the author in 1910 (West Lafayette, Ohio) and its suppression in 1917, and perhaps another 100,000 copies of Kirkpatrick’s *Think or Surrender* (New York, 1916) were distributed. Angel’s *Great Illusion* (London, 1913) had a great following among internationalists, including those who reluctantly supported the war.

lying cause of the war, they agreed, but its catalyst was nationalism itself. In this sense, at least, all the belligerents shared the blame for spreading the war and for the injustice and deceit that characterized it. Indeed, Jane Addams, Kirby Page, and others found that their reports of Allied atrocities were resented by the public simply because it accepted that notion of exclusive national virtues that had led to war in the first place. Pacifists distinguished between the mean motives of all belligerent governments and the high idealism of all the peoples who fought, as did Woodrow Wilson, but they could not support Wilson's idealistic war on behalf of the Allies. Everything they knew of the war's origin pointed to a strictly nationalistic European conflict with which the United States had no business. Private business (war trade and finance) was involved, to be sure, but neither national security nor American ideals were entrenched on one side or the other of no-man's land.

American pacifists were not intentionally isolationist in this regard. They consciously identified with men from all belligerent nations who shared what John Haynes Holmes called an "international mind." Holmes had matched a brilliant record at Harvard University with vigorous leadership in the Unitarian church, where he helped to organize the Fellowship for Social Justice. In 1912 he had written of the revolutionary function of the modern church in America, and four years later he broadened his horizon to include the international scene. There, in the midst of war, he found kindred spirits in Karl Liebknecht, Romain Rolland, and Bertrand Russell, among others. With them he recognized that there were in the world intense struggles for human dignity and decency, for peace itself, but he found these issues active within each nation at war.

Some such transnational humanism characterized pacifists of every hue, from class-conscious socialists to social gospel clergymen—Walter Rauschenbusch, for example, or Paul Jones, an Episcopal bishop who was removed from his diocese because of his views. It was the organizing principle of the Fellowship of Reconciliation and of the American Friends Service Committee. The St. Louis Resolution of the Socialist party implied that American intervention was "a crime against the people of the United States and against the nations of the world."⁴² The war seemed irrelevant to the pacifists because, in short, it seemed artificial. Neither side epitomized the values in which pacifists believed, whether phrased socialistically or religiously. No victory promised political justice or the quality of life for which they had labored as progressives. For this reason the famous and radical reporter John Reed wanted to tell the soldiers of both sides, "This is not your war."⁴³ For this reason Max Eastman found the war "un-interesting for all its gore" and Bertrand Russell called it "trivial, for all its vastness."⁴⁴

This interpretation of the war gained the force of moral commitment from

⁴² Quoted in Fine, *Labor and Farmer Parties*, 313.

⁴³ Granville Hicks, *John Reed: The Making of a Revolutionary* (New York, 1937), 169.

⁴⁴ Eastman, "The Uninteresting War," *The Masses*, VI (1915), 5-8; Russell, *Justice in War Time*, quoted in Holmes, *International Mind*, 13.

the values that pacifists held. It set them apart from prowar internationalists (just as it would set them apart from isolationists after the war). One after another they described the anguish of being isolated in the midst of idealism about the war. They were able to endure only through the fellowship of other pacifists and their activity for war relief and civil rights.

But, in fact, many did more than endure. The generation of leaders whose pacifism matured between 1914 and 1919 were "as a man . . . awakened out of sleep," suddenly alive to the "moral confusion and disorder that lie concealed in a civilization heavily weighted with materialistic aims."⁴⁵ Heightened social responsibility and a more radical view of society led some men to participate in the labor movement after the war. A. J. Muste, for example, joined the strikers, was general secretary of the Amalgamated Textile Workers until 1921, and then became director of Brookwood Labor College and started his sojourn with radicalism. Other pacifists, including Norman Thomas, Devere Allen, and Kirby Page, were led toward active socialism. The sources of this leftward shift were varied; one was the pacifists' association with antiwar socialists, and another was their confrontation with the wartime state.

The radical peace and justice movement of the post-1914 era was international from its inception. A similar devotion to pacifism and social work by religious men and women from London, Berlin, Paris, and Prague led to Quaker relief projects and the creation of the International Fellowship of Reconciliation. A similar conception of the war's origin and of peace terms linked American pacifists with British left-wing labor, antiwar German socialists, and the Russian Petrograd Council. In the United States the chief agencies of this first united front were various civil liberties bureaus and the People's Council of America.

Launched at a huge Madison Square Garden rally May 30, 1917, the People's Council was organized by moderate socialists and the remnant of the progressive antiwar coalition. Its original program was familiar enough: from May to September it campaigned for a quick peace on liberal terms, for civil liberties and repeal of conscription, and for economic demands no more radical than fair labor standards, curbs on the high cost of living, and taxes on war profits. Hoping to supplant its socialist-pacifist base with a farm-labor coalition, its organizers formed local branches and affiliated labor groups. By August it claimed just under two million constituents, a measure of its aspirations more than of its power. Five large meetings were held across the country as the Council prepared for a grand constituent assembly on September 1.

Clearly, the Council was associated with international socialism on war issues. Just as clearly, it was billed as radical and subversive by fervent patriots and

⁴⁵ Typed ms., unsigned, ca. 1917, a draft of a statement on behalf of the Fellowship, probably by Norman Thomas or Paul Jones, FOR Papers, SCPC. This sense of recognition appears not only in the literature of the Fellowship, but also among Quakers such as those in the Friends Service Committee and the Philadelphia Yearly Meeting Committee on the Social Order.

conservative labor leaders, who thwarted its plan to meet in Minneapolis. Amid great confusion, delegates aboard a special train from New York pulled into the one Midwest city willing to be their host—Chicago. Even there the meetings were hasty and almost covert. Throughout the fall the Council increasingly represented socialists and radical labor; it was the chief defender of Soviet Russia in 1918–19, but it never quite lost the marks of liberal progressivism. Its program remained virtually unchanged. Scott Nearing, a socialist economist who was dismissed from the Wharton School of Finance because of his reform activities, became chairman on the understanding that the Council would work for “industrial democracy,” but when he was asked if that meant socialism, he replied, “No.”⁴⁶ Nonetheless, pacifists who associated with socialists, in the Council or elsewhere, were tarnished with the radical image.

The nature of that so-called radicalism is important: it derived from the reflection of pacifists upon wartime society in terms of their own experience. Isolation was painful enough, but pacifists were, in fact, the target of persecution because of their opposition to conscription and their association with political radicals. They promised not to obstruct the war effort, but their skeptical neutralism was itself a crime. Pacifists found that their meetings were broken up; their friends were harassed, run out of town, and imprisoned; their literature was withheld from the mails; their headquarters raided; and the president they trusted kept his own peace. Early in the war, before nationalism was virulent, the People’s Council printed in facsimile a Russian peace appeal, together with an English translation and this note: “The original copy of the Bulletin from which this reproduction is made was smuggled over to this country—though not, as in the old days—smuggled *out of Russia*, but, as in these strange, new days—smuggled *into America*!”⁴⁷ Pacifists now looked upon their earlier warnings as prophetic. The American Union had said in 1916 that “militarism is the real danger” of the war, and Randolph Bourne was not alone two years later in describing the “inextricable union of militarism and the State,” or in fearing that “War is the health of the State.”⁴⁸

Bourne assumed that the ruling classes use the instruments of the state and its military authority to exploit those whose allegiance it commands. There was nothing new in his description of economic injustice or even its connection with war, but he went on to identify violence as the essence of war and authoritarianism as the essence of the state. In a state that identifies itself with democracy, the

⁴⁶ Minutes of the executive committee of the People’s Council, Sept. 21, 1917, People’s Council of America Papers, SCPC. See also the minutes of the organizing committee, June 21, July 19 and 26, and Aug. 16, 1917, and the *Bulletin* of the People’s Council, Aug. 7, 1917, p. 1, *ibid*. The assembly of September 1, 1917, is fully documented in the People’s Council Papers. The best published versions are in Frank L. Grubbs, Jr., *The Struggle for Labor Loyalty: Gompers, the A. F. of L., and the Pacifists, 1917–1920* (Durham, 1968), and Peterson and Fite, *Opponents of War*, although these authors interpret the Council and also the American Union Against Militarism as being somewhat more radical than the manuscripts seem to warrant.

⁴⁷ *Bulletin*, Aug. 7, 1917, pp. 2–3.

⁴⁸ Bourne, “The State,” in *War and the Intellectuals*, 89, 84.

authority of conformity takes the place of violent, physical force. Wartime patriotism is, therefore, the obverse, the domestic counterpart, of military force. Violence and authoritarianism are essentially and equally objectionable.

Bourne made the most significant statement of this theme, but it was echoed in the diverse literature of liberal pacifism, introducing a political and ethical note into the antiwar socialism of Max Eastman, Scott Nearing, and Norman Thomas. Only three weeks before he applied for membership in the Socialist party, Thomas had written that he feared its tendency to bind the individual to the class.⁴⁹ He did join because he feared more deeply "the undue exaltation of the State" and believed that "radicals ought to stand up and be counted."⁵⁰ He was a radical pacifist before he was a socialist, and his distrust of violence and authoritarianism would leave its mark upon the party in the future. Scott Nearing was becoming politically more radical in these years, but he declared that, in the name of liberty and humanity, he was against violence in any cause. Max Eastman was no absolutist—like most socialists he was against World War I specifically—but his fervor against that war modified his radicalism. Later he recalled, "A similar thing happened . . . to a good many American socialists. The reality of armed conflict in Europe dampened the proletarian-revolutionary part of their credo, and stepped up to a high pitch the antimilitary part."⁵¹ They emerged all the more skeptical and alienated from society.

Bourne's understanding was reflected, too, in Kirby Page's influential analysis of war as the method of violence and in the declarations of pacifists in the Fellowship of Reconciliation. Their enemy was war itself, and they concluded that war was the result of the entire competitive economic system. War could be linked to the whole "causal circle," wrote Vida D. Scudder, a socialist professor at Wellesley who had ferreted out the social ideals of English literature, and the pacifist who saw this connection would be forced into a "constructive social radicalism."⁵² Under the circumstances it was radical enough to express skepticism of the war or the social system of which it was a part. When conformity is an instrument of war, as in 1917–18 it was, then skepticism is a crime. The liberal pacifists stood accused as a group. In their alienation they discovered that what made their pacifism radical was their equal objection to violence and authoritarianism.

This discovery pointed toward a new ethic of conflict, one that looked for the implications of war as a method and related the instrument to its objectives. As

⁴⁹ Thomas to Mrs. Anne C. Brush, Sept. 24, 1918, Thomas Papers, NYPL.

⁵⁰ Thomas to Alexander Trachtenberg, Oct. 18, 1918, and Thomas to Morris Hillquit, Oct. 2, 1917, Thomas Papers, NYPL.

⁵¹ Eastman, *Love and Revolution*, 26; Henry May, *The End of American Innocence: A Study of the First Years of Our Time, 1912–1917* (New York, 1959), 368.

⁵² Scudder, *On Journey* (New York, 1937), 285. Miss Scudder joined the FOR after she realized that its members were integrating their pacifism with a demand for drastic social reorganization. Similarly, Jane Addams' autobiographical works can be read profitably as records of recognition of injustice on expanding levels of the social order, culminating in a view of the relationship of international injustice and war.

Kirby Page wrote, war must be judged by what it does because that is inseparable from what it is for. John Haynes Holmes argued at length that although the logic of force is that it can defend and liberate men, the fallacy of force is that it actually brings new forms of conflict and is the *sine qua non* of tyranny. Jane Addams spoke repeatedly of the futility of using violence in order to deal with the causes of fighting. "Militarism can never be abolished by militarism," as the majority of socialists had agreed in St. Louis when the American government determined to "make the world safe." "Democracy can never be imposed upon any country by a foreign power by force of arms."⁵³ Their declaration was directed specifically to the international war and was based on a Marxist analysis, but it reflected the very liberal values that absolute pacifists took to imply a universal principle.

The religious pacifists of the FOR abjured fighting on the grounds that it is sinful in its consequences, and agnostic Max Eastman found himself mindful of the "mangled bodies and manic hatreds implied by that lyric word *violence* so dear to humdrum petty-bourgeois dreamers like George Sorel. . . ."⁵⁴ For a generation and more pacifists would evaluate choices in terms of the relationship of "ends and means" and, in fact, the phrase would acquire a sanctity independent of tough-minded analysis. The new pacifist ethic was not fully articulated in its inception, in part because the war was brief; the fetters of conformity were shortly removed, and professional peace advocates felt free again to fight militarism without, it seemed, challenging the state.

The memory of World War I was an important consideration in the responses of Americans to foreign affairs for two decades. It was a formative influence upon the pacifist. He tended to universalize the war and apply its example to other events; he used it to popularize his view that wars are always futile and irrelevant to fundamental social issues and that the United States could stand aside from a European state system based on force of arms. Revisionist histories of the First World War provided a vehicle for inculcating that view, but they could not convey the internationalism that was a corollary of the pacifist's humanism. To the extent that his memory of the war was accepted by the public, it encouraged isolationism.

There was a deeper dilemma. Pacifism was historically oriented to liberal values. The progressive background of the liberal pacifist reinforced these values even as it socialized them and added a disposition toward political action. In 1917-18 the pacifist began to view war as an integral part of an unjust social order. The instruments of political control involved at least the latent threat of violence, he discovered, and these were in the hands of classes opposed to change. Behind even the system of democratic majority decision he found the tacit

⁵³ Majority report of the Socialist party, St. Louis, quoted in Fine, *Farmer and Labor Parties*, 312.

⁵⁴ Eastman, *Love and Revolution*, 26.

sanctions of violent force. To his political right and left were activists for whom violence appeared to be the ultimate authority.

But if the pacifist remembered anything from World War I, it was that violence and authoritarianism were precisely what threatened his every liberal value. Against them he began to define an ethic of conflict, dealing with force as an instrument for social control and rejecting violent means.

Only a few pacifists perceived that their impulse to far-reaching reform might come into conflict with their refusal to sanction violence in any cause: Evan Thomas and his friends on a hunger strike at Ft. Leavenworth, perhaps; John Haynes Holmes trying to find ways to rationalize the passivity out of pacifism, looking for the example of a Gandhi; Kirby Page seeking nonviolent methods of social change; moderate socialists warding off a Bolshevik-communist line. Even these men forgot the dilemma once the war was over, and they returned to normality or took up again the traditional instruments of social change. Liberal pacifists would face it again, however, in the agony of defining the road to power that split the Socialist party in 1934 and in the fight against war and fascism; and their successors would meet it in the sixties in the civil rights movement and the opposition to the war in Vietnam. The terms of the dilemma were exposed in World War I. A willingness to grapple with them would characterize liberal pacifism in the twentieth century.

The Revolution in English Social Thought, 1880–1914

REBA N. SOFFER

IN 1958, H. Stuart Hughes justified confining his survey of the reconstruction of European social thought to the western and central Continent by arguing that Germans, Austrians, Frenchmen, and Italians contributed more than Englishmen, Americans, or Russians to “the fund of ideas that has come to seem most characteristic of our own time.”¹ This tendency to neglect English social thought in the generation before the First World War fits well with the prevailing view of the late Victorian and Edwardian periods as a time of decline, a view given now almost classic form by George Dangerfield and newly reiterated by Samuel Hynes, who sees that period as a time of “undifferentiated rebellion.” But is it true that, as J. W. Burrow has recently concluded, “England made no distinctive contribution to the rethinking of the fundamental concepts of social thought” in the early twentieth century?² On the contrary, from the 1880’s until 1914 there was in England a genuine, vital revolution in the contents, methodology, and purposes of social thought. An inductive, behavioral social science bent upon effecting practical social reform overthrew a deductive social theory that assumed inherent laws of human nature and society.³

By the end of the nineteenth century, a second industrial revolution, social dislocation, agricultural decline, unemployment, and mounting discontent exerted a persistent and accelerating pressure upon traditional social theory. Throughout the nineteenth century the development of methods of social inquiry testifies to recurrent, though disparately motivated, efforts to reduce complex social phenomena to more manageable, often quantitative form.⁴ What these efforts

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¹ H. Stuart Hughes, *Consciousness and Society: The Reorientation of European Thought, 1890–1930* (New York, 1965), 13.

² George Dangerfield, *The Strange Death of Liberal England* (New York, 1935), viii; Samuel Hynes, *The Edwardian Turn of Mind* (Princeton, 1968), 9; J. W. Burrow, *Evolution and Society* (Cambridge, 1966), 260.

³ Philip Abrams argues that from its beginnings in 1834 British social science was directed toward policy. When energies were absorbed wholly in administration, because there was no “point of entry to the centers of British social and intellectual life,” sociology “languished.” Abrams, *The Origins of British Sociology: 1839–1914* (Chicago, 1968), 149. On the contrary, the influential and active membership of the Sociological Society and the contributors to the *Sociological Review* testify to the closeness between dominant figures in social and intellectual life and social scientists of all kinds.

⁴ The most perceptive, comprehensive account of the nineteenth-century attitude toward social inquiry appears in R. K. Webb’s unpublished “The Victorian Sense of Fact.” I am greatly indebted to Professor Webb for generously making this paper available to me.

lacked, especially after mid-century, was a satisfying theoretical explanation to provide meaning beyond the specific project investigated. Until the end of the century some "social scientists" abandoned theory altogether to flounder in mere facts, while others, who gathered facts in confirmation of a priori assumptions, moved increasingly away from social reality. The creation of an empirical social science was retarded before the 1880's by the lack of empirical social theory. The deductive models that had dominated nineteenth-century social theory were unable to predict, explain, or influence events.

In the 1880's the new social sciences started with the moralistic idealism and progressivist assumptions of their predecessors, but they transformed these assumptions through a new methodology to create a social science in which theory and practice would supplement and correct each other. A moralist's conviction that individual and social life should be more rational and more humane, and the observation that neither reason nor humanity were increasing, led the new social scientists to reinterpret progress as the deliberate activity of informed individuals guided by a probabilistic science derived from experience. As moralists, they were compelled to retain progressivist assumptions; altruism, good will, social responsibility, all the desirable ethical characteristics could be realized only if people and institutions were capable of changing for the better.

The reforming dedication of the new social scientists was shared at the end of the nineteenth century by other individuals and groups, notably Beatrice and Sidney Webb and the Fabian Society. The Webbs and other Fabians devised conspicuously successful techniques of investigation, research, and influence, but their impatience with theory, their indifference to problems of methodology, and their retention of utilitarian psychology and ethics belonged to an older tradition of social science. Method to the Webbs meant that procedure, generally an elitist manipulation of institutions and their personnel, that would accomplish specific goals of reform most efficiently. While the Webbs were concerned essentially with performance, the new social scientists probed deeply into individual motives and those social and economic conditions responsible for behavior.

The new social sciences were, to use T. H. Kuhn's suggestive hypothesis about the structure of scientific revolutions, the "tradition-shattering complements to the tradition-bound activity" of "normal" or prevailing views of society, economics, and politics.⁵ The revolution had been heralded by a number of works that rejected the methodology and epistemology popular in theoretical and applied social science. In the economics of the 1870's, W. Stanley Jevons and Alfred Marshall had arrived independently at an analysis of "utility" that broke decisively with the classical system;⁶ and Jevons in 1874 and Karl Pearson in 1892 had argued that economic and social problems should be treated through sophisticated prob-

⁵ Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962), 5. "Normal" is used by Kuhn to describe that activity that the particular professional "community acknowledges for a time as supplying the foundation for its further practice" (p. 10).

⁶ T. W. Hutchison, *A Review of Economic Doctrines, 1870-1929* (Oxford, 1953), 14.

ability theory.⁷ William James's earliest analysis of consciousness, the first severance of epistemology and psychology in England, appeared in *Mind* in 1879.⁸ Although these new doctrines were debated strenuously in the 1870's and 1880's, they did not have a significant impact upon the wider intellectual community until 1890.⁹

In 1890, the publication of Alfred Marshall's *Principles of Economics* and William James's *Principles of Psychology* was welcomed enthusiastically by intellectuals and an excited press as a revolution in the social sciences. These two systematic expositions of content and method immediately became the definitive classics in their fields, virtually unchallenged in Britain for a generation. Marshall and James imposed behavioral tests upon their new disciplines and each inspired critical disciples who carried the new faith to problems that had eluded nineteenth-century intellectuals and activists alike.¹⁰ Then, in 1908, Graham Wallas created

⁷ Jevons, *Principles of Science* (3d ed., London, 1879), 149–50; Pearson, *The Grammar of Science* (London, 1892), 136–80.

⁸ In addition to "Are We Automata?" (1879) and "The Sentiment of Rationality" (1879), James's most important early papers—"On Some Omissions in Introspective Psychology" (1884), "What is an Emotion?" (1884), "Absolutism and Empiricism" (1884), "On the Function of Cognition" (1885), "The Perception of Space" (1887), "The Psychological Theory of Extension" (1889), and "The Psychology of Belief" (1889)—all appeared in the British journal, *Mind*. James often lived in England and he was accepted into the British intellectual aristocracy. His central concept of the necessary relation between thought and feeling was first presented to the British "Scratch Eight," which included Edmund Gurney, Frederick Pollock, Leslie Stephen, Frederick William Maitland, Carverth Read, and Shadworth Robinson. The other two members, G. Croom Robertson and James Sully, were not present. Although James had taught psychology at Harvard from 1875 and had signed a contract for a book on psychology, he made no significant progress on the book until he worked out the Scratch Eight paper. Gay Wilson Allen, *William James* (New York, 1949), 264–67. It may be argued that James's influence in Britain was greater than his immediate influence within the United States.

⁹ Supply and demand had been treated as functions of price by A. A. Cournot as early as 1838, but the concept was first made popular by Marshall's *Principles of Economics* (Cambridge, 1890). Joseph Spengler, "On the Progress of Quantification in Economics," *Isis*, LII (1961), 267. Jevon's work, important for its use of probability theory, its definitions of marginal utility and value, and its rejection of the labor and cost of production theories (especially in his *Theory of Political Economy* [London, 1871]), was still conceived in the classical belief that "the theory of economics proves to be, in fact, the mechanics of utility and self-interest," in which the "laissez-faire principle properly applied is the wholesome and true one." Jevons, "The Future of Political Economy," written in 1876, in *The Principles of Economics: A Fragment of a Treatise on the Industrial Mechanism of Society, and Other Papers* (London, 1905), 199, 203. To Jevons political economy was nothing more than "the science of wealth." Jevons, *Primer of Political Economy* (London, 1878), 13. In technical analysis, especially price analysis, and in his view of the interdependence of economic phenomena, Marshall went far beyond Jevons. It might be argued that John A. Hobson's economics were more "revolutionary" than Marshall's, especially his theories of "underconsumption" or the "maldistribution" of cyclical unemployment, which appeared in *The Psychology of Industry*, written with A. F. Mummery (London, 1889), *Problems of Poverty* (London, 1891), and *Problem of the Unemployed* (London, 1896). But Hobson never reached the audience that rushed to sit at Marshall's feet.

¹⁰ John Maynard Keynes, often credited with the "new economics" of the 1930's, attributed the significant revolution to his mentor, Marshall. J. M. Keynes, "Alfred Marshall," in *Memorials of Alfred Marshall*, ed. A. C. Pigou (Cambridge, 1956), 24–27. As Professor of Economics at Cambridge from 1885 until the 1920's, Marshall trained such influential economists as W. H. Beveridge, Lajo Brentano, J. H. Clapham, E. R. A. Seligman, and Joseph A. Schumpeter, to mention only a few. "Address to Marshall on his Eightieth Birthday (July 26, 1922) from Members of the Royal Economic Society," in *Memorials*, 497–99. Until 1929 theoretical economics in England and in the United States consisted very largely of "the discussion and interpretation" of Marshall's *Principles*. Hutchison, *Review of Economic Doctrines*, 62. See also Frank Knight, "Economics," in *On the History and Method of Economics* (Chicago, 1963), 21; and Joan Robinson, *Economics is a Serious Subject* (Cambridge, 1932), 8. Marshall contributed to the problems of his interpreters by constantly revising the *Principles* until the eighth edition in 1920. Marshall, *Principles*, annot. C. W. Guillebaud (9th [variorum] ed., London, 1961), II, 15–30. Increasingly after 1890, Marshall moved away from his reforming purposes of the mid-1860's through 1890 and accepted existing conditions as progressive, especially in his *Industry and Trade* (London, 1919). This paper is concerned only with the younger

a revolution in political thought by applying Marshall's quantitative method and James's pragmatic psychology to the study of politics in his *Human Nature in Politics*.¹¹

Technical analysis, quantitative and comparative methodology, and a tentative theoretical structure were all revolutionary, but not as revolutionary, perhaps, as the ends toward which the new social sciences were directed. Economics, psychology, and political science were conceived of as efficient methods for improving and reforming human nature and society. In common, Marshall, James, and Wallas set out to make social science an ordering tool for imposing rational and moral imperatives upon the given anarchy of economic, social, and political conflict.

The new social science repudiated a pervasive social theory that had developed in response to the unsatisfying, transient, unreliable, and most seriously, unpredictable qualities of experience. Instead of reforming experience to make it more satisfying, nineteenth-century theorists had attempted to abstract principles of continuity from its changing attributes. Disturbed and puzzled by the succession of rapid changes in Victorian society, they hoped to discover social laws that would render experience intelligible and predictable. Even militant opponents of theory like the statisticians, who explicitly disavowed questions of cause and effect as legitimate concerns for social inquiry, implicitly believed that hidden laws governed society.¹² A psychological desire to find reality ordered led thinkers as disparate as John Stuart Mill, Walter Bagehot, Henry Thomas Buckle, Herbert Spencer, and T. H. Huxley to seek basic truths about human nature and society from which a descriptive social science could be deduced. Stanley Jevons had observed in 1874 that a scientific or "truly philosophic" mind cannot tolerate doubt because it is "the confession of ignorance, and involves a painful feeling of incapacity."¹³ The form and content of nineteenth-century social science had been set

Marshall. All citations are from the 1890 edition of the *Principles*. James was Graham Wallas' friend (Wallas' unpublished papers at the London School of Economics and Political Science contain some of their correspondence), and the first chapter in Wallas' *Human Nature in Politics* (London, 1908) was derived largely from James's *Principles of Psychology* (New York, 1890). F. S. C. Schiller's *Humanism* (London, 1903) was based upon James's pragmatism; and Bertrand Russell learned many of his most characteristic doctrines from James. John Passmore, *A Hundred Years of Philosophy* (2d ed., London, 1966), 232-62. For a discussion of the reception of James's *Principles*, see Allen, *William James*, 323-26, and Ralph Barton Perry, *The Thought and Character of William James* (briefer version, New York, 1964), 196.

¹¹ Wallas, *Human Nature in Politics* (3d ed., New York, 1921), 206. All subsequent references will be to this edition.

¹² In the first issue of the *Journal of the Statistical Society* (1838), statistics was described as a science which "neither discussed causes nor reasoned upon probable effects, but sought only to collect, arrange and compare . . . facts." Quoted in Abrams, *Origins of British Sociology*, 15. But the Belgian astronomer, L. A. J. Quételet, the mover behind the formation of the London Statistical Society in 1834, hoped that statistics would reveal the kind of laws characteristic of astronomy. Paul Lazarsfeld, "Notes on the History of Quantification in Sociology," *Isis*, LII (1961), 278; Harold Westergaard, *Contributions to the History of Statistics* (London, 1932), 174. The collection of physical and moral statistics by the British statistical societies was hardly aimless. Quételet's a priori concept of "l'homme moyen," a social, physical, and moral ideal, was arrived at by confusing statistical averages with desirable attributes. Many statisticians set out to find a numerical warrant for this ideal type.

¹³ Jevons, *Principles of Science*, 592. Arthur James Balfour's *Theism and Humanism* (London, 1915) was to be based upon a similar psychology of doubt.

by a question intended to avoid such "incapacity": "What are the discernible laws that govern social life?" There was debate on the rigor of social laws, but theorists and activists alike agreed that social reality was governed by fundamental, inherent principles.

Physical science supplied nineteenth-century social theory with an epistemology and a method for reducing the overwhelming complexity of experience to more satisfying proportions. As late as 1870 even so radical an economist as Jevons insisted that the fruitful line of development for the social sciences was as the "necessary complement to the physical sciences."¹⁴ Physical scientists understood nature as a permanent and predictable entity governed by universal laws. When physicists in the 1920's were dismayed by their colleagues' interminable debates, they looked back to nineteenth-century physics as an "age of correlation"¹⁵ in which phenomena, which had seemed to be "capricious and isolated," were brought into "consistent and comprehensive order."¹⁶

The physicist's quest for ideal and typological invariants was virtually unchallenged until Darwinian biology, a study of individuals differing from one another at any given time as well as through time, insisted upon the importance of historical origin and individuation. Natural selection, as a process of adaptation, provided an entirely different kind of scientific model derived from individual, clinical, and developmental criteria. The biologist induced general propositions from observation and comparative processes that impressed upon him, as Huxley pointed out as early as 1854, the "utterly conditional nature of all our knowledge." But Huxley believed that such conditionality was relative to time rather than an essential attribute of knowledge itself.¹⁷ The telling argument against biological procedures yielding absolute knowledge came from the quantitative methods and assumptions implicit in Darwin's conclusions.¹⁸ By the end of the nineteenth century, William James attributed to the theory of evolution an "entirely new quantitative imagination" that had "swept over our western

¹⁴ Jevons, "Opening Address as President of Section F [Economic Science and Statistics] of the British Association for the Advancement of Science," in *Methods of Social Reform and Other Papers* (London, 1904), 189. By 1880, Jevons had modified his public position to argue that, while social reforms could be designed on the basis of generalized theory, "specific experience on a limited scale and in closely approximate circumstances is the only sure guide in the complex questions of social science." "Experimental Legislation and the Drink Traffic," in *ibid.*, 265. But, in his most serious work, *Principles of Science*, originally published in 1874, he had identified "scientific genius" as the "ability to discover the one in the many" (p. 626).

¹⁵ Paul R. Heyl, *Fundamental Concepts of Physics in the Light of Modern Discovery* (Baltimore, 1926), 28.

¹⁶ Florian Cajori, *A History of Physics in its Elementary Branches (through 1925)* (rev. ed., New York, 1962), 142.

¹⁷ Huxley, "On the Educational Value of the Natural History Sciences," in *Science and Education* (New York, 1964), 54. While the inductions of the mathematician had been formed and completed and he was "occupied now with nothing but deduction and verification," the biologist "deals with a vast number of objects, and his inductions will not be completed . . . for ages to come," but when they are, biology will be as "deductive and exact" as mathematics (p. 56). Huxley viewed biology as the midpoint between the physico-chemical and the social sciences.

¹⁸ Charles Gillispie argues convincingly that although not numerical in expression, Darwin's work was "quantitative" in method and matter of thought because selection determined the quantity of living things that could survive in any given set of objective circumstances. *The Edge of Objectivity, An Essay in the History of Scientific Ideas* (Princeton, 1960), 339.

world" within his generation. Evolutionary theory "now requires us to suppose a far vaster scale of times, spaces, and numbers than our forefathers ever dreamed. . . ."¹⁹

Social theorists did not turn to biological models until after 1880. Then, the anomalies of experience made existing social theory glaringly insufficient. The physicist's promise of the increasing unity of knowledge had been relevant only to those who wanted to believe that the overwhelming disunity of experience was subject to a coercive order that existed behind events. Social reformers who applied themselves to specific social problems often implicitly held the same *a priori* assumptions that the social theorists had borrowed from physics. This was especially true of that umbrella reform organization, the National Association for the Promotion of the Social Sciences, founded in 1856 to discover optimal measures for reforming individual character and those social diseases that perverted character. Its major conviction—that society was a rational consensus of interests willing and able to agree upon and implement social remedies—assumed an underlying order to be discovered by the investigatory techniques of social science.²⁰ These reformers, with their untenable burden of assumptions, were as ill-equipped as the deductive theorists to survive the growing economic and social discontent of the 1880's.

Social theorists seeking a comprehensive conceptual structure might have turned to mathematics or medicine for their models, but until after World War I, they did not. Certainly the educational system singled out mathematics as the discipline most appropriate to intellectual training; and medicine, taught and practiced outside the educational establishment, was particularly successful in dealing with the intimidating problems of public health. Neither mathematics, despite its dominance at the universities, nor medicine with its practical victories, ever rivaled the influence that physics, and then biology, exerted upon social theory.

Although social theorists never turned directly to mathematics to provide them with either a method or an epistemology, mathematics did serve them indirectly in a special way. As long as mathematics was essentially a paradigm of ordered explanation, it was the highest form of physics and the purest form of thought. In 1854 George Boole equated the laws of thought to the laws of mathematics to explain that deductive generalizations were given validity by the "ability in our nature to appreciate Order, and the concurrent presumption, however founded, that the phenomena of Nature are connected by a principle of Order." The general propositions of mathematics and thought, unlike the collected facts of experience, were "*necessary*" truths.²¹ When mathematics explained the phenomena

¹⁹ James, *Human Immortality* (Boston, 1898), 32-33.

²⁰ Abrams presents a perceptive analysis of the origins, development, and failure of the NAPSS in *Origins of British Sociology*, 39-52.

²¹ Boole, *An Investigation of the Laws of Thought on which are Founded the Mathematical Theories of Logic and Probabilities* (London, 1854), 403, 404, 422.

of nature, the implications were mechanistic and deterministic. This was true even of James Clerk Maxwell's and Ludwig Boltzmann's laws of statistical mechanics. These statistics had nothing to do with experience or induction; on the contrary, they were an a priori deductive imposition of mathematical order upon the difficult physical quantities of heat and entropy.²²

Until medicine became concerned with causality, it did not provide models readily adaptable to social theory. Innocent of any knowledge of causes, medicine treated only symptoms. On rare occasions generalized causal relations were identified, such as William Farr's isolation of a London water company as the source of the cholera epidemic of 1866; but discovery of the specific cause of cholera had to await the development of bacteriology.²³ Nineteenth-century medical study and practice were primarily restorative rather than preventative; the cure of a specific malady was intended to restore a previous condition rather than to anticipate a future condition of increased well-being. This organic emphasis upon continuity was not especially congenial to social theorists committed to progressivist assumptions or to those seeking the causes underlying social phenomena. Medical practice taught only one lesson of importance to social theorists: the utility of quantification as an instrument of social observation,²⁴ a lesson implicit in all the new genetic sciences. William James rejected his own medical training because it appeared to him to be a superficial mode of soothing suffering through palliatives based largely upon ignorance;²⁵ and the founders of British social psychology, William McDougall and Wilfred Trotter, rejected their medical training and practice to build a social theory on a priori assumptions and a deductive methodology.²⁶

Until the last two decades of the nineteenth century, most educated people believed that proper "science" was, like physics, a system of absolute and inflexible rules that scientists discover.²⁷ This lesson was reinforced whenever scientists philosophized about their purposes to an audience larger than their immediate colleagues. The public lectures delivered by eminent English scientists at the Royal Institution in 1854 to plead the "important cause of Scientific Education" are a striking instance of the way in which the scientific community created faith in a given order behind experience.²⁸ None of the lecturers related scientific education to ordinary affairs or practical needs. On the contrary, the

²² Kurt Mendelssohn, "Probability Enters Physics," in *Turning Points in Physics* (Amsterdam, 1959), 54-67.

²³ William Farr's quantitative methods of inquiry as registrar-general were extended by his successors in the 1880's and 1890's to relate health and occupation. Westergaard, *Contributions to the History of Statistics*, 213, 216, 250-51.

²⁴ Richard H. Shryock argues that the use of quantification in medicine, most conspicuous in public health, is as an instrument of social observation. "The History of Quantification in Medical Science," *Isis*, LII (1961), 237.

²⁵ For James's letters complaining of his medical studies, see Allen, *William James*, 98, 121.

²⁶ Reba N. Soffer, "New Elitism: Social Psychology in Prewar England," *Journal of British Studies*, VIII (1969), 111-40.

²⁷ Herbert Spencer, *Social Statics* (London, 1851), 42; Noel Annan, *Leslie Stephen* (London, 1951), 144.

²⁸ *Lectures on Education, delivered at the Royal Institution of Great Britain* (London, 1855), 6.

study of science was valued as an intellectual pleasure, achieved by demonstrating consistent inner lines of connection in the physical world through elegant mathematical models and clever demonstrative reasoning.²⁹ Although there was no nineteenth-century dichotomy between theoretical and applied science, the purpose of science was to discover structure, not to impose it. Even Boole, almost unique among mathematicians in his view of the laws of nature as strictly "*probable* conclusions," still insisted that "the business of science" is "not to create laws but to discover them."³⁰ Science was not considered as a manipulative procedure until the twentieth-century revolution in physics. No nineteenth-century scientist could have legitimately spoken of "inventing the future."³¹

To the nineteenth century, the order of reality was permanent; it could not be compelled through science to be other than it was. John Tyndall, one of the most important popularizers of nineteenth-century science, described scientific inquiry as the procedure for understanding the "visual record of the Creator's logic."³² The spirit of the Royal Institution lectures of 1854 was captured by William B. Hodgson's definition of science as the "order which binds the parts into a whole," or more simply as "the pursuit of *Law*."³³ "Law" was a rational description of the fixed, uniform, and inevitable qualities of a static physical universe. Only Michael Faraday, the self-educated head of the Royal Institution, brilliantly and uniquely successful as an experimental physicist without mathematical training, warned against the illusion of absolute and complete knowledge: "In drawing a conclusion it is very difficult, but not the less necessary, to make it *proportionate* to the evidence: except where certainty exists (a case of rare occurrence) we should consider our decisions as probable only."³⁴ It is true that Faraday's ignorance of mathematics compelled him to represent physical reality through physical models rather than by means of abstract mathematical formulas. But his skepticism about deductive theory came from his belief in laboratory experiment as the means for testing conclusions, from his conviction that natural laws were simply summaries of existing levels of knowledge, and from his Sandemanian belief that absolute truth was an attribute exclusive to God.³⁵

²⁹ Lord Kelvin never completely accepted Clerk Maxwell's brilliant mathematical model for describing electro-magnetism, but he could not help admiring its "beauty." Kelvin, "Presidential Address to the Royal Society, November 30, 1893," in *Popular Lectures and Addresses* (London, 1894), II, 547.

³⁰ Boole, *Investigation of the Laws of Thought*, 4. Boole made a distinction between the essentially probabilistic character of natural laws and the absolutely certain laws of logic and mathematics (p. 11).

³¹ W. S. Fowler, *The Development of Scientific Method* (Oxford, 1962), 101. See also Max Planck, *The Philosophy of Physics*, trans. W. H. Johnston, in his *The New Science* (New York, 1959), 286.

³² Tyndall, "On the Importance of the Study of Physics as a Branch of Education for All Classes," in *Lectures on Education*, 210.

³³ Hodgson, "On the Importance of the Study of Economic Science as a Branch of Education for All Classes," in *ibid.*, 264. Hodgson was an educational reformer and eventually the first Professor of Political Economy and Mercantile Law at the University of Edinburgh.

³⁴ Faraday, "Observations on Mental Education," in *ibid.*, 71-72.

³⁵ L. Pearce Williams' definitive biography uses Faraday's letters, lectures, and private papers extensively to demonstrate his skepticism. Williams, *Michael Faraday* (London, 1967), 89, 105-06,

Faraday's caution was not accepted by the scientific establishment until the end of the century.³⁶

Throughout the second half of the nineteenth century, physicists, popularizers of science, and philosophers created an intellectual atmosphere receptive to the belief that social laws were the analogies of physical laws and of an order higher than mere empirical generalizations.³⁷ This dichotomy between a causal science and empirical experience was established by the authority of J. S. Mill's *Logic* (1843).³⁸ Mill had been torn between his desire for a social science based upon causal laws on the one hand and his need to trust individual moral responsibility on the other. Mill recognized that the scientific process began and ended with experience, but the effect of his treatment of social laws was to leave them as generalized and deterministic principles. Mill used demonstrative reasoning to reduce problems of individual behavior and social process to tendencies not easily distinguishable from deductive physical laws. Once the basic social postulates were understood, social and individual tendencies could be predicted, Mill believed, with substantial accuracy. A sincere moralist and a liberal advocate of social change, he contributed, unwittingly, to a tradition of social science suspicious of change. His treatment of the mechanics of change as regular and continuous, though not yet adequately grasped, did not invite individual reforming activism. Although Mill tried to modify the causal qualities of his social "tendencies" by assigning moral responsibility to individuals for self-direction,³⁹ he never succeeded in reconciling causal social process with the role of individual choice and conduct. In his 1851 revision of the *Logic*, he reduced the compulsion of social laws further by arguing that instinct, too, was susceptible to willed "mental influences" and education.⁴⁰ As rector of Saint Andrews University in 1867 he emphasized simultaneously the determined conditions of existence and

335-36. Faraday was an elder in the Sandemanian Church, a sect that taught that God moved the heart beyond the powers of science or logic. *Ibid.*, 106. See also comments by Tyndall, Faraday's successor at the Royal Institution, in his book printed in 1868, *Faraday as Discoverer* (New York, 1961), 180; and see *The World of the Atom*, ed. Henry A. Boorse and Lloyd Motz (New York, 1966), I, 318. Faraday doubted atomism and action-at-a-distance because neither could be proven experimentally. *Ibid.*, 315-21.

³⁶ Faraday's tentative assessments were tested in the laboratory of the Royal Institution from 1825 to 1867, where, as the successor to Humphrey Davy, he was one of the few genuinely experimental scientists of nineteenth-century England. Edmund T. Whittaker, *A History of the Theories of Aether and Electricity: The Classical Theories* (London, 1958), 197 n. 56.

³⁷ An older, but less important, "scientific" tradition during the nineteenth century (developed by Charles Babbage, inventor of the computer, Lucasian Professor of Mathematics at Cambridge from 1828 to 1839, and a founder of the Statistical Society in 1834) argued that physical laws were no more than statements of probability. Boole and Jevons belong in this tradition. But nineteenth-century social scientists found the absolute implications of demonstrative laws more persuasive. Henry Thomas Buckle approved Mill's *Political Economy* as a "branch of political knowledge which is not empirical . . . the only one raised to a science." Buckle to Miss Shireff, July 5, 1855, quoted in A. H. Huth, *The Life and Writings of H. T. Buckle* (New York, 1880), 89.

³⁸ Mill, *A System of Logic* (London, 1843). William James tried to reconcile this dichotomy through pragmatism as a via media between the "empirical thinker" who "stares at a fact in its entirety, and remains helpless, or gets 'stuck'" and the rationalist with his a priori assumptions. *Principles*, II, 330.

³⁹ Mill, *A System of Logic*, II, 485.

⁴⁰ *Ibid.* (1851), II, 431.

the need for individual effectiveness in a battle between good and evil, which changed as human nature and society changed.⁴¹ But as long as the "fundamental problem" of social science was to find the laws of progress upon which society evolved, individualistic morality was left little influence.

A belief in causal laws led other social scientists, who declared themselves free of all "systems of opinion or doctrine," to find evidence for progress well into the 1890's.⁴² Even T. H. Huxley, notoriously skeptical about the reality of progress, admitted by 1886 that the growth of all science, including social science, "means the demonstration of order and natural causation among phenomena which had not previously been brought under these perceptions."⁴³ When Huxley opened the new Scientific College at Birmingham in 1880, he urged that sociology apply the methods of physics to show "that social phenomena are as much the expression of natural laws as any others."⁴⁴ The ordering models that social theorists took from physics satisfied esthetic and intellectual cravings for an internally consistent system derived logically from deductive postulates, but at the expense of their ability to control and improve given conditions. A longing for social order and its persistent elusiveness in experience led nineteenth-century social theory into a blind alley.

When Victorian social theorists accepted the concept of causality, they were uneasy, as Mill had been, about its deterministic implications. Many of the most dramatic results in nineteenth-century science had implicitly undermined free will by emphasizing a deterministic causality. Hermann Helmholtz's theory of the conservation of energy excluded individual free will completely, while the study of reflex action in physiology indicated, at least to T. H. Huxley, that behavior might in fact be completely automatic.⁴⁵ The most damaging evidence against individual freedom was marshalled in Quételet's "moral statistics." In his *Sur l'Homme* (1835) he had argued that figures showed that the constancy of crime seemed "quite independent of human foresight."⁴⁶ Although Quételet was never entirely convinced that individual freedom was impossible, many of his readers, like George Boole, were carried away by his startling statistics to conclude that where large numbers of men were involved, there was a "very remarkable degree of regularity in their behavior."⁴⁷

Statistics had been used as an account of causality by Laplace before Quételet,

⁴¹ *Idem*, "Inaugural Address at St. Andrews," in *Dissertations and Discussions* (New York, 1874), IV, 158, 197.

⁴² This was particularly true of the use of statistics. Abrams, *Origins of British Sociology*, 26-30.

⁴³ Huxley, "Science and Morals," in *Evolution and Ethics* (New York, 1899), 130.

⁴⁴ *Idem*, "Science and Culture," in *Science and Education*, 139-40.

⁴⁵ *Idem*, "On the Hypothesis that Animals are Automata," in *Collected Essays* (New York, 1917), I, 198-250.

⁴⁶ Quoted in Passmore, *A Hundred Years of Philosophy*, 36. See also Lazarsfeld, "History of Quantification in Sociology," 309.

⁴⁷ Boole, *Investigation of the Laws of Thought*, 21. Boole did concede that an individual's free will was not "inconsistent with regularity in the motions of the system of which he forms a component unit," but what impressed him, and Quételet's other readers, most was that statistical records contained "the seeds of general truths" buried "amid the mass of figures" (p. 21).

and it was used in this way by Clerk Maxwell to explain the thermal behavior of molecules. But Maxwell was disturbed about the limiting effect of such a model, and in 1873 he urged scientists to study the "singularities and instabilities rather than the continuities and stabilities of things," to dispel that "prejudice in favour of determinism which seems to arise from assuming that the physical science of the future is a mere magnified image of the past."⁴⁸ If "averages," or "general laws," or "predetermined tendencies," were applied to social phenomena, then individual differences were rejected. William James worried considerably about individuals who were tempted to relinquish their standards of value to the misleading appearances of inevitability.⁴⁹

In the middle of the nineteenth century, as well as at the end, theorists were deeply interested in questions of value, and the gap between the ideal and the real was lamented loudly. Yet social theory based upon deterministic assumptions permitted thought and conduct to survive, even when they failed to meet normative criteria. Few nineteenth-century theorists were willing to allow the past to determine the structure of the present and the future.⁵⁰

Nineteenth-century social theory had attempted to treat the ideal and the real as if they were complementary parts of knowledge and experience. Many individuals, responding to various religious, humanitarian, or psychological imperatives, wanted to act constructively against conditions unacceptable to them. To provide room for their ethical view of individual activism within a larger causal scheme, they accepted a progressive view of history. If the individual could perceive the progressive processes, then he could move more rapidly on an escalator inevitably, but slowly, ascending. If free will were an agent for a greater good, then the incompatibility between social causality and individual reformism disappeared; the individual could overcome the psychological defeat resulting from the realities of adversity by believing that his purposes, because progressive, were irresistible in the long run. Such a rationalization was comforting, but it could not do away with the moral dilemma of using ends to justify means. Moreover, this reconciliation, in theory and in practice, depended entirely upon agreement that progressive forces did indeed move history.

When, in the last half of the nineteenth century, the notion of beneficent forces began to be challenged by Darwinian biology, reflex studies in physiology, a re-enforced mechanism in physics, and the erosion of religious conviction, causality remained as an attribute of history affecting man but not necessarily

⁴⁸ Clerk Maxwell, "Does the Progress of Physical Science tend to give any advantage to the Opinion of Necessity (or Determinism) over that of the Contingency of Events and the Freedom of the Will?" reprinted in Lewis Campbell and William Garnett, *The Life of James Clerk Maxwell* (London, 1882), 444. This paper was read to the Eranus Club, men who had been "Apostles" together from 1853 to 1857.

⁴⁹ See James's "The Importance of Individuals," written in 1890, in *The Will to Believe and other Essays in Popular Philosophy* (London, 1897), 262.

⁵⁰ Even Herbert Spencer, the most zealous advocate of positivism, modified the force of social law by individual action. See especially his "Progress: Its Laws and Cause," in *Essays: Scientific, Political, and Speculative* (London, 1858), 1-54.

in his interest. If the processes of history were neutral, then what were the options open to individuals unwilling to accept the determinism of prevailing tendencies? Some Victorian social theorists tried to make moral voluntarism and invariable social laws consistent by assigning them different levels of influence: Charles Kingsley's inaugural address as Regius Professor of History at Cambridge in 1860 admitted broad patterns in history, but he made progress depend completely upon individuals acting according to conscience.⁵¹ But Kingsley was no more successful than Mill; the unity of knowledge and reality presupposed by causal laws was contradicted by his assumption of free will. None of the nineteenth-century theories of ethics and society ever explained why actual behavior and institutions deviated so widely from the idealized form postulated through abstract social laws.

Victorian theories about social and political behavior were normative or psychological; they either discussed human nature as they wanted it to be or as they thought it actually was. Neither normative theory, derived from rationalist biases, nor psychological precepts, rooted in the intuitionists' "common sense," was sufficient to explain behavior.⁵² Both schools were dissatisfied with prevailing ethics, but neither could find any ethical imperative in its views of social process. Eventually both schools were compelled to appeal to the individual's ethical conscience, although with serious qualms about the efficacy of such an appeal.

At the end of the century, an infusion of Hegelian idealism reduced the ethical dilemma of social theory to an internal conflict between a higher and a lower self. The evangelical struggle between flesh and spirit was translated by T. H. Green into a social conflict between individual rights and political obligations. The retention of both positive social laws and individualistic morality had been most ambitious in Herbert Spencer's attempt to devise a physical science of ethics. Green pointed out that the effect of Spencer's reconciliation would be an acceptance of current behavior as if it were the only behavior possible.⁵³ Instead, Green's ethics contrasted the unsatisfactory quality of existing behavior with superior standards that ought to exist.⁵⁴ Oxford idealism, influenced by Green, asserted that social structure and ethical value must be imposed by the mind.⁵⁵ At Cambridge, G. E. Moore persuaded a brilliant generation of undergraduates that ethics were neither predetermined nor governed by natural laws.⁵⁶ Al-

⁵¹ Kingsley, *The Limits of Exact Science as Applied to History* (London, 1860), 17-44.

⁵² William Whewell was the most persistent and influential intuitionist, and J. S. Mill the most important of the rationalist psychologists, particularly in his desire for a social science based upon individual control of character, as well as environment. See Mill's reply to Adam Sedgwick's intuitionist argument in "Professor Sedgwick's Discourse—State of the Philosophy of England," *Westminster Review*, XXX (1835), 94-135.

⁵³ Green, *Prolegomena to Ethics*, ed. A. C. Bradley (5th ed., Oxford, 1907), 3-12.

⁵⁴ See Melvin Richter's discussion of T. H. Green's philosophical idealism in *The Politics of Conscience* (London, 1964), 176.

⁵⁵ Francis H. Bradley's *Ethical Studies* (1876) contained a similar kind of social and ethical theory revolving around the idea of "self-realization" (2d ed., Oxford, 1962), 79-81. See also Bernard Bosanquet, *The Philosophical Theory of the State* (London, 1965), 310.

⁵⁶ Moore, *Principia Ethica* (Cambridge, 1956), 43. Keynes's account of Moore's influence appears in Roy F. Harrod, *The Life of John Maynard Keynes* (London, 1951), 75, 78, 80.

though the social and ethical theories developed at Oxford and Cambridge were disparate in content and purpose, their point of congruence lay in a rejection of the values perpetuated by history as a sufficient source of judgment.

By the close of the century, social reformers in search of supportive social theory found little inspiration in the unresolved contradictions between positivism and experience. Nor could they persuade themselves, as many of the antitheoretical reformers of the National Association for the Promotion of the Social Sciences and the various statistical societies had done, that social and moral progress would be demonstrated if sufficient statistics were accumulated. After 1880, social theorists rejected assumptions of progressive teleology to find their methods in progressive epistemology, but the old philosophical debate between rationalism and empiricism continued to divide them.

Rationalism had been bolstered throughout the nineteenth century by the typological and ideal physical sciences. Although challenged by an increase in experimental science at the end of the century, rationalistic physics re-emerged in 1905 in the newer and grander structure of relativity.⁵⁷ Empiricism, always second in the nineteenth-century debate, had been made more reputable by the "quantitative" procedures of Darwinian natural science, by the success of statistical methods in practical policy questions, especially in public health, and by the rising pressures of social problems whose solutions could not be postponed indefinitely. Rationalism and empiricism met at the end of the century in agreement that "facts" were a function of mind. No one was willing to believe that "facts are merely *recognised* by a mind, not *made* by it."⁵⁸ This unwillingness to accept a reality independent of mind was endorsed by the new sciences of biology, psychology, anthropology, and much of sociology in their emphasis upon origins rather than essences.

When reformers after 1880 adapted a theoretical structure to give coherence to their reforming impulse, their choice of method and theory was governed by their assumptions about human nature and society. The rationalistic or deductive model attracted theorists who valued stability more than free will, while the empirical or inductive model appealed more to advocates of experimental change and individual choice. The social psychologist William McDougall used deductive and axiomatic principles to find the individual a regrettable deviate from the increasingly ordered process of evolution toward a supra-collective mind able to overcome the unpredictable and irrational impulses that drove every individual.⁵⁹ Graham Wallas, the pioneering political scientist who applied

⁵⁷ Until Lord Rayleigh took over the Cavendish Laboratory at Cambridge in 1879, Clerk Maxwell's appeal in 1871 for the encouragement of experimentation had hardly been answered. R. B. Lindsay, "Historical Introduction," in Lord Rayleigh, *The Theory of Sound* (New York, 1945), I, viii. Although experimental work became essential to physics, portions of Albert Einstein's deductive system have still not been proven experimentally.

⁵⁸ Passmore, *A Hundred Years of Philosophy*, 175. Passmore traces the agreement from Mill's associative mechanisms through T. H. Green's notion that facts are constructed by thought, F. H. Bradley's version of facts as man's distortion of reality, to James's and Henri Bergson's view that they are the mind's tools for dealing with experience.

⁵⁹ McDougall, *Psychology: The Study of Behavior* (London, 1912), 105, 242-43; "The Will of the People," *Sociological Review*, V (1912) 99, 101.

the perceptions of Marshall and James to political problems, used the inductive model to gather damning evidence against individuals unwilling to choose social, economic, and political forms genuinely responsive to their needs.⁶⁰

Those people who, at the end of the nineteenth century, attempted to develop a specialized science of man that combined both theory and practice did not act collectively, nor did they see themselves as having common goals. There were others, such as the Webbs, too preoccupied with practice to inquire into theory; their eclectic methods were meant only to achieve particular results. To these ends, they adapted convenient social science tools without pausing to question the larger nature and contents of social science. Those who did attempt to create a professional social science, with the exception of sociology, which was torn into conflicting fragments, may be put into three distinct categories for the sake of analysis: reformers, revisionists, and traditional positivists. Each of the three believed that they were pioneers on the brink of fundamentally new discoveries about man in society, but only the reformers can be called "new" social scientists. They were the only ones who genuinely succeeded in creating "new" methods, contents, and purposes for the social sciences.

The traditional positivists were the least influential of the three groups. Edward Westermaarck, co-recipient with L. T. Hobhouse of the first chair of sociology at the University of London from 1904 to 1907, and the anthropologist E. B. Tylor, were the most successful spokesmen for the traditional view.⁶¹ Westermaarck and Tylor used empirical research to verify their conclusions, but their plethora of data was superficial window dressing for a familiar stock of a priori social concepts. Selective research supported their original conviction that the past, present, and future were necessary and continuing stages in the desirable development of man and his institutions.⁶² The comparative sociology and anthropology influenced by Tylor and Westermaarck studied development to discover those conditions specific to social progress and conducive to individual action. In practice their "empirical" methodology was simply a crude application of evolutionary precepts to a reassuring history of morality. The traditional positivists provided familiar testimonials to individual progress from irresponsible irrationality toward mature reason.⁶³

The revisionists, too, and especially the social psychologists, wanted moral conduct to improve, but they could find no evidence of the growth of social

⁶⁰ See Wallas, *Human Nature in Politics*, 198-214.

⁶¹ Hobhouse's sociology belongs in the newer or reforming tradition of social science. See his *Morals in Evolution* (New York, 1906), II, 280.

⁶² See Idus L. Murphee, "The Evolutionary Anthropologists: The Progress of Mankind: The Concepts of Progress and Culture in the Thought of John Lubbock, Edward B. Tylor, and Lewis H. Morgan," *Proceedings of the American Philosophical Society*, CV (1961), 265-300; George Stocking, "'Cultural Darwinism' and 'Philosophical Idealism' in E. B. Tylor," *Southwestern Journal of Anthropology*, V (1965), 140-41.

⁶³ Edward Westermaarck, *The Origin and Development of the Moral Ideas* (London, 1906), II, 739-45. Edward B. Tylor saw history as progressive emancipation from irrational and primitive "survivals" from the past. *Primitive Culture* (3d ed., London, 1891), I, chap. 3. McDougall began his social psychology with a healthy suspicion of evolution's effects upon man's morality, but the effects of his own destructive critique led him eventually to seek security in a higher plane of evolution not unlike Westermaarck's modification of utilitarian psychology by evolutionary theory.

morality in evolutionary history. On the contrary, they were alarmed by the values and conduct that evolution appeared to perpetuate. Beginning with an irrationalist view of human nature and an organic image of society governed by inherent laws of development, they rejected emphatically the rationalist aspirations of a priori sociology. When they tried to reconcile their understanding of morality, as the obligation to be reasonable, equitable, and altruistic, with their pessimistic view of human nature, they failed. Their distrust of human nature drove them into a modified and elitist version of the positivist social theory they had set out to repudiate.⁶⁴ Revisionist social scientists preferred the security of universal laws to the threat of individual irrationality. Eugenacists belonged to this tradition too. They wanted to manipulate heredity, but for ends that conformed to organic institutions and values. Their only criticism of the status quo was that it lacked sufficient efficiency.⁶⁵ All the revisionists wanted to reject rationalist psychology and teleology, but their "science" continued to be modeled upon the rational persuasiveness of demonstrative argument. The effect of revisionist social science was political elitism and conservative social theory.

The development of sociology vacillated unsuccessfully between traditional positivism, revisionism, and the newer social science until after the First World War. In 1877, G. J. Shaw-LeFevre, in his presidential address to the London Statistical Society, reminded his colleagues that the "science of Sociology" was "essentially a deductive" one, concerned with causality. The following year, J. K. Ingram, president of the Economic Science and Statistics Section of the British Association, urged that the section be converted into a Sociology Section based upon an inductive methodology. When Henry Sidgwick was president of that section in 1885 he completely rejected traditional positivism and sociology to argue that it is "our business to carry on more limited and empirical studies of society. . . ."⁶⁶ After the founding of the Sociological Society in 1904, debates upon method and purpose grew vehement. Sides were drawn between practical social administrators and theoreticians like L. T. Hobhouse, editor of the *Sociological Review*, who believed that the most immediate problem for sociologists was the development of theory. This culminated in a divisive crisis in 1911 that was not reconciled until the 1920's.⁶⁷

The genuine revolution in the social sciences began after 1880 in response to the inadequate content of existing social theory, the inadequate condition of social reality, and the inability of nineteenth-century social science to deal with these inadequacies systematically and successfully. The immediate effect of the revolution was to change the problems considered appropriate to social science

⁶⁴ See Soffer, "New Elitism," 111-40.

⁶⁵ Francis Galton wanted to improve the racial qualities of man within a society that was a "highly complex organism, with a consciousness of its own." Galton, *Foundations of Eugenics* (London, 1907), 25.

⁶⁶ All quoted in Abrams, *Origins of British Sociology*, 80-81, 83.

⁶⁷ The crisis in the Sociological Society resulted from the conflicting purposes of the Society's members. These appear in the pages of the *Sociological Review* from 1908 to 1914.

by enlarging the narrow concern of earlier reformers with individuals or small groups and by limiting theoretical analysis to the specific social anomalies that demanded immediate and critical attention. The potential political power of a barely literate democracy, the multiplication of social, economic, and political hostilities between individuals and groups, and the growing guilt among intellectuals about responsibility for these conditions led the new social scientists to reject the continuing influences of the past upon the present, whether ideological or institutional.

The new social scientists started with sanguine expectations about their ability to explain, and eventually to solve, the anomalous character of experience. Their optimism came from the novelty of their specialized program and, most important, from a moralistic faith that truth and right must prevail against error and evil. Their science, based upon tentative assessments of experience, would impose progressive direction upon random social processes.⁶⁸ Retaining a psychological and moral trust in science as an objective method for distinguishing truth from error, the new social scientists restricted scientific method to the measurement, observation, and comparison of data in actual time and place.

Measurement was not a new concept in social science, but its use at the end of the century was entirely different. In his provocative analysis of revolutions in science, T. H. Kuhn suggested that the function of measurement is either to precipitate a crisis by exposing anomalies in prevailing theory or to aid in the choice between competing theories.⁶⁹ This analysis is not characteristic of the social sciences today, but it can be applied to the positivistic social science ascendant until the 1880's.⁷⁰ Measurement did precipitate a crisis by disclosing the inability of positivist theory to analyze accurately problems of behavior and organization, and it did aid the emerging social sciences to resolve the conflict between rationalistic and empirical epistemology and deductive and inductive method.

Although the new social scientists tried to restrict their analysis to measurement, their reforming ambitions spilled over the limits of the methodology they had adopted because they never thought of themselves as objective and disinterested students of academic problems. They were passionate partisans of individual well-being—moral, mental, and material—to be achieved within a liberal, democratic society. Service to ethical ends motivated Marshall's economics, James's psychology, Wallas' political science, Hobhouse's social philosophy, and Seebohm Rowntree's sociology of poverty. When James argued in 1879 that the individual could not remain neutral or skeptical in moral questions, he was voicing the pervasive reforming ethic of his time.⁷¹ A commitment to a muscular

⁶⁸ Burrow, in his instructive book *Evolution and Society*, finds the key to early twentieth-century thought in the "severance of social and political theory" (p. 276). But the new social scientists expected their science to change both social and political institutions.

⁶⁹ Kuhn, "Measurement in Modern Physical Science," *Isis*, LII (1961), 181.

⁷⁰ In an appendix to "Measurement in Modern Physical Science," Kuhn readily admits that the lack of agreement about subject and method in contemporary social science excludes the application of his model (191-93).

⁷¹ James, "The Sentiment of Rationality," in *The Will to Believe*, 109.

progressivism, a disciplined exercise of will and reason, was pervasive in reforming thought and conduct at the turn of the century. Until after World War I, social science was expected to provide guidance for ethical progress, to tell "what might be" and "what ought to be."⁷²

The new social scientists were clear about their ethical obligation. They were less certain of the methods best suited to effect those obligations. In parallel though unrelated movements, the physical sciences and a new mathematics were developing revolutionary methods. The growing emphasis upon experiment in the physical sciences supported the social scientists' reliance upon experience, but the new mathematics was entirely irrelevant to their theoretical or methodological needs. Traditional physics, demonstrated through a mathematics understood as the most concise and truest expression of physical reality, was challenged in the last quarter of the nineteenth century by the extension of experimental facilities, beginning with the foundation of the Cavendish Laboratory at Cambridge in 1871,⁷³ and by the growth of a new mathematics cut away completely from any reference to the empirical. The trend of nineteenth-century mathematics in England, from Boole through W. K. Clifford, John Venn, and Bertrand Russell, was toward an equation of mathematics and logic, an analysis of internal consistencies rather than an attempt to discover truths, simply to know "what can be deduced from what."⁷⁴ Experiment, as a proof of a priori principles, or as an inductive process leading to the formulation of new principles, compelled even the most deterministic science to undergo empirical tests at some point. But in the new mathematics, algebra no longer dealt with quantities, non-Euclidean geometry was no longer concerned with spatial relationships and figures in nature, and even arithmetic studied "trans-finite" numbers that could not be counted.⁷⁵

The new social scientists were keenly interested in these changes fermenting in other disciplines, both to confirm their own directions and to indicate possible pitfalls. Inductive procedures required in experiment became central to their method, but they found no use for a mathematics indifferent to experience. They were interested in a mathematical representation of quantitative and comparative data and in the refinement of statistics and probability as methods of

⁷² Raphael Mendola, "Evolution: Darwinian and Spencerian," in *Herbert Spencer Lectures at Oxford: Decennial Issue, 1905-1914* (Oxford, 1916), 38.

⁷³ James Clerk Maxwell became the first Professor of Experimental Physics and the head of the laboratory. Clerk Maxwell never confirmed his theoretical structure through experiment, although other physicists eventually did so. Whittaker, *History of the Theories*, 240-75.

⁷⁴ Bertrand Russell, "Mathematics and the Metaphysicians," in *The World of Mathematics*, ed. James R. Newman (New York, 1956), III, 1578.

⁷⁵ For a complete discussion of the history, content, and implications of the new mathematics, see *ibid.*, 1576-90. In England, non-Euclidean geometry was advocated enthusiastically by W. K. Clifford, the brilliant Cambridge mathematician, popularizer of science, and militant agnostic, in his important paper to the British Association in 1872, "On the Aims and Instruments of Scientific Thought," *Lectures and Essays*, ed. Leslie Stephen and Frederick Pollock (London, 1879), I, 136-41; and his *Common Sense of the Exact Sciences*, ed. Karl Pearson (New York, 1946), 203-04. After Boole and into the twentieth century, the major work in the new mathematics was done on the Continent.

comprehensive psychological, social, economic, and political investigation. One of the earliest applications of the new mathematical techniques to social problems was the eugenicists' development of probability theory. The new social scientists strenuously opposed the eugenicists' subversion of theory and method to a priori, exclusively elitist ends.⁷⁶ James, Marshall, and Wallas would not use any tool that disposed of the disarray of experience by imposing a deductive structure upon it.

Since Laplace in 1820, probability had developed as a form of deductive thought. Probability theory could have been used, as the social scientists were to do, to originate or to prove limited generalizations, but instead probability became a means for subjecting manifestations of chance to predictable laws. Influenced by Quételet's statistics, George Boole had tried in the 1850's to devise a deductive general theory of probability that would permit a social scientist to make successful predictions. Jevons, influenced by Boole, interpreted probability to mean that there are no necessary and sufficient causes. This implied clearly that order or natural laws resulted from fortuitous and random circumstances. The same lesson was repeated in the conclusions of Darwinian biology, atomic physics, and a revival of Mendelian biology. It was left to the mind to impose order upon chance through techniques of deductive mathematical probability.

Probability theory was applied to the a priori ordering of social chance in Francis Galton's *Hereditary Genius* (1869), an attempt to establish intellectual achievement as a direct function of heredity. Galton adopted Quételet's normal law of error to measure ability without questioning whether this law actually explained the conditions in the real world. By 1889 Galton had found this law to be "a wonderful form of cosmic order." The "huger the mob, the greater the apparent anarchy, the more perfect is its sway. . . ." Galton confused the structure imposed by his theoretical tool with the actual conditions he set out to investigate; he marveled how, in large samples of chaotic elements, "an unsuspected and most beautiful form of regularity proves to have been latent all along. The tops of the marshalled rows form a flowing curve of invariable proportions, and each element, as it is sorted into place, finds, as it were, a pre-ordained niche, accurately adapted to fit it."⁷⁷ Pearson added a more developed concept of correlation, "broader than causation," to enable the social sciences, like the physical sciences, to be treated through mathematics.⁷⁸ This tradition, challenged with conspicuous

⁷⁶ James, Marshall, and Wallas relied upon improvements in social and economic conditions, increased opportunity, and widespread education to produce healthy and rational men. See especially James's famous chapter on "habit," written in 1877, in his *Principles*, I, chap. 4; his *Talks to Teachers on Psychology, and to Students on Some of Life's Ideals* (New York, 1900); Marshall's *Principles*, chaps. 4, 5; his "The Future of the Working Classes" (written in 1873), "Where to House the London Poor" (1884), "A Fair Rate of Wages" (1887), "Co-operation" (1889), "Social Possibilities of Economic Chivalry" (1907), all in *Memorials*; and see Wallas, *Human Nature in Politics*, 305.

⁷⁷ Galton, *Natural Inheritance* (London, 1889), 66.

⁷⁸ Pearson, "Autobiographical Sketch," in *Speeches Delivered at a Dinner Held in University College, London, in Honor of Professor Karl Pearson, 23 April 1934* (Cambridge, 1934), 19. Pearson began as a liberal socialist whose sympathies were moved toward an elitist and conservative position by the Boer War.

success by the new social scientists, continued to have its adherents. Raphael Mendola, the Herbert Spencer Lecturer for 1910, told his audience that even when there were enough observed facts to permit their quantitative expression, the important conclusions in science were "arrived at deductively by men who have never carried out an experiment or made an observation—through the most powerful and potent of all weapons—mathematics."⁷⁹

The new social scientists selected from contemporary developments in method and theory what best served their reforming purposes. The range of their choice appeared, initially, to be restricted by the recalcitrant traditions persisting in educational and professional institutions. But the most persistent tradition, a preference for amateurism, actually facilitated the revolution in the social sciences by compelling social scientists to develop independent traditions and to define their own competence, method, and purpose. The prolific contributions of amateur science throughout the nineteenth century, most remarkable in T. H. Huxley's successful efforts to be equally proficient in a staggering variety of subjects, had encouraged men without a formal preparation to pursue a wide range of scientific problems actively and often effectively. But the new social scientists saw that the intensive development of their disciplines must rely upon consistent professional training and practice: amateurism was more a liability than a liberating force. Even by the end of the century and with few exceptions, professional standards were neither rigorously defined nor enforced by professional bodies.⁸⁰ Educational administrators, too, were unwilling to consider specialized studies essential to their curricula.⁸¹ In the absence of compelling professional traditions, the new social scientists were able to resist the dominant positivist canons established in the public schools, universities, and "scientific" associations. The parameters of these institutions, their isolated and aristocratic values, were, in any case, too narrow for the new social scientists who wanted to reach the greatest number of people.⁸² If progress depended upon values, as the new social scientists

⁷⁹ Mendola, "Evolution," 41. A generation later, the Herbert Spencer Lecturer for 1933, Albert Einstein, affirmed that "the axiomatic basis of theoretical physics cannot be extracted from experience but must be freely invented" by "purely mathematical constructions" and that we can discover "the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena." Einstein, "On the Method of Theoretical Physics," in *Essays in Science* (New York, 1934), 17.

⁸⁰ The amateur continued to be the dominant mover in British educational and professional institutions. See A. W. Coats, "The Origins and Development of the Royal Economic Society," *Economic Journal*, LXXVIII (1968), 369; Beatrice Edgell, "The British Psychological Society 1901-1941," *Bulletin of the British Psychological Society*, Suppl. (1961), 5; O. J. R. Howarth, *The British Association* (London, 1931), 93, 301-04; Keynes, "Alfred Marshall," 54; F. H. Lawson, *The Oxford Law School, 1830-1965* (Oxford, 1968), 36-37; W. J. Reader, *Professional Men* (London, 1966); Sheldon Rothblatt, *Revolution of the Dons* (New York, 1968); 248, 258; *Royal Commission on the Civil Service, Appendix to the First Report of the Commissioners and Appendix to the Second Report of the Commissioners* (London, 1912); and William R. Ward, *Victorian Oxford* (London, 1965), 278.

⁸¹ Although Marshall was Professor of Economics at Cambridge from 1885 until 1908, he did not succeed in establishing a separate Tripos in economics until 1903. Keynes, "Alfred Marshall," 57; Marshall, *The New Cambridge Curriculum in Economics and Associated Branches of Political Science; its Purpose and Plan* (London, 1903).

⁸² Matthew Arnold testified before the Schools Enquiry Commission that in England, with a population over twenty million, there were only 3,500 matriculated university students as compared

believed, then the problem was to discover what those values should be and how to make them available throughout society. Theology, utilitarianism, and Anglicized idealism had not, alone or in combination, been able to resolve the conflicts between individual desires and social needs. Marshall, James, and Wallas believed that only "science" could make a democratic society a moral one.

When the new social scientists tried to build their "science," they were confronted with divergent and conflicting views of scientific method, including mathematical demonstration, experimental evidence, crude impressionism, sensitive perception, and analysis of similar and dissimilar events. The only common assumption in all these views was that scientific thought, properly understood, was the means by which human progress was accomplished.⁸³ The new social scientists replaced scientific thought by a scientific method that would eventually produce practicable criteria for progressive change. An interest in refining scientific method characterized the different varieties of social thought at the close of the century. The realization that science was hindered by inappropriate methods led Karl Pearson to define method broadly as an "orderly classification of facts followed by the recognition of relationship and recurring sequence."⁸⁴ William James described science similarly as a "certain dispassionate method." While Pearson was to use biometrics as a methodological prelude to eugenic changes, James knew that if science were degraded to a particular "set of results that one should pin one's faith upon and hug forever," it would serve only sectarian purposes.⁸⁵ All the new social scientists understood method as a flexible and expanding set of procedures to observe and test experience.

The inability of nineteenth-century social theory to account for burgeoning complexity taught the new social scientists to avoid simple panaceas and comprehensive laws. Marshall's economics, James's psychology, and Wallas' political science used generalized theory only to clarify concrete cases. A theoretical structure was no more than a convenient way of arranging complicated data. James's "radical empiricism" testified to the view that everything in a plural world was hypothetical. Marshall and Wallas believed that, too.⁸⁶ The debris of population growth, industrial displacement, agricultural recession, economic depression, and human irrationality could not be eliminated by sweeping it all under a seamless carpet of theory.

If the new social scientists had discarded theory completely, then their quanti-

to 6,362 Prussian students representing a population of eighteen and one half million. *Report of the Commissioners appointed to inquire into the State of Popular Education in England* (London, 1861), VI, 633. Older scientists, such as T. H. Huxley, equally concerned to make science accessible to ordinary men, were sharply divided from the "newer" men by their rejection of value as a legitimate concern for a social science. Huxley to Thomas H. Farrer, Dec. 19, 1894, in *Life and Letters of Thomas H. Huxley*, ed. Leonard Huxley (New York, 1900), II, 407.

⁸³ Clifford, "Aims and Instruments of Scientific Thought," I, 157.

⁸⁴ Pearson, *Grammar of Science*, 23.

⁸⁵ James, "What Psychological Research Has Accomplished," in *The Will to Believe*, 119-20.

⁸⁶ See James's "Sentiment of Rationality," written in 1879 and revised in 1880, 90, 110, and "The Will to Believe," *ibid.*, 29; Marshall's *Principles*, chap. 1; Wallas' *Human Nature in Politics*, chaps. 4-5.

tative and experimental techniques would have resulted in a narrative miscellany typical of Charles Booth's statistical inquiries into the life and labor of London's poor, or in specific critiques of institutional abuses exemplified by the Webbs's treatment of the poor laws. While Booth provided a descriptive and detailed census of the "condition of England," and the Webbs provided programs implemented a generation later, neither statistics nor the Webbs's prescriptions replaced the social and economic theory they had discarded.

Booth began, as the new social scientists did, with a conviction that existing theory failed from a "want of reality" because it was based upon "a series of assumptions very imperfectly connected with the observed facts of modern life."⁸⁷ Booth's data contributed to the eventual growth of a sociology of poverty, but his work lacked systematic and critical analysis. Inspired by Booth, the Quaker industrialist Seebohm Rowntree surveyed poverty in York. He used more direct and perceptive sampling techniques, which led to specific wage and social security recommendations, but Rowntree's work lacked an adequate conceptual form.⁸⁸ The tradition begun by Booth and Rowntree produced a torrent of blue-books and government reports, which overwhelmed reformers from 1900 to 1914 with a mass of unevaluated information. The new social scientists went beyond the social investigators to use experience and theory as complementary processes to resolve crises that could not be subsumed under comprehensive laws or postponed in a passive expectation of the triumph of progressive forces.

Definition of method raised critical problems of content, meaning, and purpose. The early twentieth-century quarrel between the Eugenics Laboratory and its critics Marshall, Keynes, and Hobhouse, appeared to turn on the reliability of the statistical techniques developed by Galton, but the real issue was the eugenicists' absolute belief in heredity and their opponents' reliance upon environment. The larger Edwardian debate over measurement as the essential method of social science often became a discussion of the validity of technique to the exclusion of more essential problems of purpose. In part this reflected a growing intellectual concern for defining problems more precisely. G. E. Moore complained in 1903 that disagreement in philosophy arose because attempts to answer questions were made without "first discovering what it is which you desire to answer."⁸⁹ The social sciences suffered from a similar confusion about their appropriate functions.

The new social scientists carefully isolated the questions they wanted to answer; they had greater difficulty in selecting and verifying the data required to answer those questions. Physical and social sciences agreed by the last decade of the century that whatever was found to be "true" would be compelling for everyone who knew all the relevant data, but there was fundamental disagreement about the nature of truth. Rationalists understood truth as that that

⁸⁷ Booth, "Inhabitants of Town Hamlets," *Journal of the Royal Statistical Society*, L (1887), 376.

⁸⁸ Rowntree insisted that he wanted to "state facts rather than to suggest remedies," but he did suggest social reforms. Rowntree, *Poverty: A Study of Town Life* (2d ed., London, 1922), 360.

⁸⁹ Moore, *Principia Ethica*, vii.

cohered with the established body of belief, while empiricists required truth to correspond to given "facts" from the actual world. James, Marshall, and Wallas discarded both traditional views to consider truth as a human value.

William James insisted that our convictions about the "truth" of scientific laws were much more like a "religious faith than like an assent to a demonstration."⁹⁰ He was impressed considerably by that aspect of Karl Pearson's thought that found science "true" only in the sense of yielding a "conceptual shorthand, economical for our descriptions."⁹¹ Pearson had described science as a "mental shorthand" for the "sequences of our preceptions," a means of classification and reasoning, but not an "explanation." That a "certain sequence has occurred and recurred in the past is a matter of experience" that we call "*causation*"; that it will continue to "recur in the future is a matter of belief" that we call "*probability*." Pearson obviously influenced James's view of the tentative nature of scientific laws, but he never persuaded James of the unity of science as a progressive enterprise in which the observed relationships and sequences would fall into increasingly comprehensive formulas.⁹²

James could not find in evolution the progressive content that gave such comfort to Pearson. Instead evolutionary doctrines taught him to accept a "plastic world" in which "almost all our functions, even intellectual, are seen as 'adaptations' and possibly transient adaptations to practical human needs."⁹³ Pearson's skepticism about scientific "truth," derived from his use of probability theory, was contradicted by his faith in an inexorable idea of progress.⁹⁴ James, and the other new social scientists, treated thought, experience, and method, and judgments about them as ephemeral expressions of human needs in particular historical circumstances.

It may be that James and the others confused the definition of truth with the criterion for testing it, but his treatment of "truth" as simply a collective name for "verification processes"⁹⁵ made the old antithesis between inductive and deductive methods irrelevant to social science. Deduction, induction, quantification, and probability all became methods of verification in the Edwardian debate over measurement. To the new social scientists, "law" was a regularity of human behavior under a specific set of circumstances, a regularity that could be observed directly or whose existence could be surmised through the process of inductive

⁹⁰ James, *Principles*, II, 367.

⁹¹ See James's 1904 review of F. C. S. Schiller's *Humanism*, in *Collected Essays and Reviews* (London, 1920), p. 448; and his *Pragmatism, A New Name for Old Ways of Thinking* (New York, 1907), 57.

⁹² Pearson, *Grammar of Science*, 136, 392, 116.

⁹³ James's review of Schiller's *Humanism*, 448.

⁹⁴ Pearson, *National Life from the Standpoint of Science* (2d ed., London, 1905), 26, 43-44, 46-49, 63. The original text of 1900 is unaltered in the second edition, but three appendixes are added to argue for eugenics as the means for England's continued progress by those organic laws of evolution that guarantee the survival of the fitter community at the expense of the weaker.

⁹⁵ James, *Pragmatism*, 218. Fowler argues that James's pragmatism took scientific method a great step forward by bridging the "dichotomy between the theory of the philosopher and the practice of the scientist." *Development of Scientific Method*, 78.

reasoning. Such regularity, based upon forms of verification suited to the particular problem, meant only that individuals were likely to react in certain ways to certain situations. When the new social scientists gave up the quest for positivistic social laws, they accepted a concept of verification based essentially upon inductive probabilities. So long as men create and govern their own circumstances, inductive "laws" or principles have no deterministic content and there are no discernible limits restricting the reformer except his concept of reform.

Through systematic inquiry into the origins and nature of individual behavior in different circumstances, the new social scientists developed professional and highly specialized disciplines with distinct methodologies, plans of study, and apprenticeship requirements. Alfred Marshall, in his inaugural lecture as Professor of Economics at Cambridge in 1885, defined economic theory as a tool for reasoning "about those motives of human action which are measurable."⁹⁶ Between 1886 and 1903, Marshall measured them in quantitative and comparative studies of currency, prices, gold and silver, local taxes, fiscal policy, international trade, and poor relief. Unlike most of his successors, he was able to apply his method to practical problems with appreciable success: the inquiries he conducted had direct effect upon government policy.⁹⁷

Fundamentally discontented with existing conditions, the new social scientists intended their science to be a prolegomenon to change, defeating the determinism of chance and other actively regressive forces. William James's pluralism, the heart of his philosophy and psychology, developed in opposition to the idea that an individual is forced to act, or to believe, in any special way because of given circumstances. Apart from James, British and American prewar psychology remained either exclusively epistemological in its analysis of consciousness or mechanistic in its explanation of behavior. Mechanistic psychology adopted a robot model to explain behavior as essentially reactive, responding innately or through learning, to imposed stimuli.⁹⁸ James would not accept "automaton-theory" with its "*a priori* and *quasi* metaphysical grounds" because he viewed behavior as a consequence of consciousness, "at all times primarily a *selecting* agency."⁹⁹ Free will was "true" in James's evaluative judgment of truth, because

⁹⁶ Marshall, "The Present Position of Economics," in *Memorials*, 158. Economic theory was, in the next years, built anew mainly by university economists. Hutchison, *Review of Economic Doctrines*, 30.

⁹⁷ Marshall's *Official Papers* (London, 1926), published for the Royal Economics Society, includes his contributions to official inquiries on economic questions. In the 1890's there was a great "burgeoning of quantification" in the investigation of economic problems. Spengler, "Progress of Quantification in Economics," 269. Although Spengler attributes this to many factors, Marshall's influence and example were essential.

⁹⁸ In a recent UNESCO survey on main trends of research in the social sciences and humanities, Ludwig von Bertalanffy asserts that it was only after 1950 that psychological theory rejected the automaton model to treat man "as an *active personality* system." Bertalanffy, "General Theory of Systems: Application to Psychology," in *The Social Sciences: Problems and Orientations, Selected Studies* (The Hague, 1968), 309. This ignores James's influential rejection of positivist-mechanist psychology.

⁹⁹ James, *Principles*, I, 138, 139.

of its meliorative content as a "general cosmological theory of *promise*" with the same fruitful function as any other "doctrine of relief" that promised individuals a better future.¹⁰⁰ Marshall's synthesis of history and analysis in his *Principles* tried to delineate those economic conditions requisite to successful meliorative change, while Graham Wallas' synthesis of psychology and politics exposed the forces retarding such change.

Even though no reassuring faith in individual progress could be extrapolated from the events of the late nineteenth century, all the new social scientists were determined to retain individual, rational, and moral criteria for progressive change. An insistence that science serve ethical ends prevailed among late nineteenth- and early twentieth-century reformers, despite the diversity of their ends; but the new social scientists had no beneficent movers like evolution, progress, reason, or God, to rely upon ultimately.¹⁰¹ The most probing question we are ever asked, James wrote in his *Principles*, is: "Will you or won't you have it so?"¹⁰² Marshall and Wallas accepted this implicitly. As an economist, Marshall asked: "How far is it possible to remedy the evils of the present day?"¹⁰³ while Wallas taught about individual vulnerability to manipulation and the practical courses to overcome it.¹⁰⁴

The new social scientists, although they did rely upon individual commitment as a spur to reform, recognized that individuals live in complex groups that restrict their activity. It was not apathetic individuals but a lack of organization that created basic social problems. The new social scientists' analysis of history as a gratuitous complex of random forces led them to believe that planning would defeat irrationality and inhumanity. They disagreed about immediate priorities, but they shared the conviction that no part of society could be left to develop haphazardly. Their emphasis upon organization obscured the more fundamental problem of the immediate ends that specific organizations ought to serve. Increased rationality and morality may be valuable precepts, but they are not organizing principles.

Just before World War I, Graham Wallas found thought, literature, and politics permeated by a "pervasive fear, conscious or half-conscious, that the civilization which we have adopted so rapidly and with so little forethought may prove unable to secure either a harmonious life for its members or even its own stability."¹⁰⁵ Wallas was afraid that the fallibility of reason and will, which he had revealed convincingly in his pioneering study, *Human Nature in Politics*, would dis-

¹⁰⁰ *Idem*, *Pragmatism*, 119, 120.

¹⁰¹ Christian Socialists who became economists, like the Rev. Wilfrid Richmond, trusted in the benevolence of God to further their efforts; more secular reformers like McDougall, and eventually Pearson, trusted to evolution; while even a voluntarist like Hobhouse discovered certain reassuring tendencies in historical development. Richmond, *Christian Economics* (London, 1911); McDougall, *Psychology*, and "The Will of the People"; Pearson, *National Life*; Hobhouse, *Morals in Evolution*.

¹⁰² James, *Principles*, II, 576.

¹⁰³ Marshall, "The Present Position of Economics," 171; "Social Possibilities of Economic Chivalry," 324.

¹⁰⁴ Wallas, *Human Nature in Society*, 211.

¹⁰⁵ *Idem*, *The Great Society* (New York, 1921), 14.

courage intellectuals from assuming social and political responsibilities. Wallas' experience in politics, his careful scrutiny of actual political behavior, and his repudiation of the intellectualist fallacies perpetuated within deductive social and political theory led him further than either Marshall or James. Both Marshall and James had denied the reality of compelling social laws, but they had been unwilling to question the efficacy of individual reason and will to overcome irrational compulsions and an inadequate society. James's exploration of the nature of belief should have made him more sensitive than Marshall to instinctual influences upon thought and behavior. The evolutionary relation between instinct and habit, the educational role of imitative instincts, and the identification of "sociability and shyness" as important instincts never led James to conclude that instinct determined social, economic, or political behavior.¹⁰⁶ Although James understood mind to be, like Nature, "a real jungle, where all things are provisional, half-fitted to each other, and untidy,"¹⁰⁷ he persisted in believing that reason could convert jungles into Cambridge gardens.

Wallas never confused his reformist ideals with actual conditions; his faith was as great as the other new social scientists, except that it began in greater skepticism. Through a devastating critique of the ethical and rationalist psychology sufficient for Marshall, and even James, Wallas initiated a behavioral political science concerned as much with human nature as with circumstances. Wallas accepted T. H. Green's moral axioms and tried to find behavioral criteria for converting those axioms into practical beliefs. He understood, as Marshall and James did not, that even the best-intentioned sense of political obligation had little opportunity for effectiveness unless armed with psychological knowledge of the motives and conditions influencing actual conduct. James had described psychology as "the most powerful ally of hortatory ethics,"¹⁰⁸ but he had never suggested any practical application of this principle. Marshall, too, believed that psychology and ethics were inseparable and essential to any reform. Torn between economics and psychology, Marshall had chosen economics because of the "increasing urgency of economic studies as a means towards human well-being."¹⁰⁹

At the beginning of the new century, the Corpus Christi Professor of Jurisprudence at Oxford described the dominant concern of his time as a "craving for a scientific treatment of the problems of social life."¹¹⁰ This craving could not be satisfied by social theory that reduced complex phenomena to a few simple laws. When Marshall, James, and Wallas set functional limits to specific social problems, they changed the scientific model accepted since the 1840's as most ap-

¹⁰⁶ James, *Principles*, II, 402-30.

¹⁰⁷ *Idem*, "Frederick Myers's Services to Psychology," *Memories and Studies* (London, 1911), 166.

¹⁰⁸ *Idem*, *Principles*, I, 127.

¹⁰⁹ Marshall to Professor James Ward, Oct. 23, 1900, in *Memorials*, 418-19.

¹¹⁰ Paul Vinogradoff, *The Teaching of Sir Henry Maine* (London, 1904), 10-11.

propriate for all forms of scientific inquiry. The new social sciences circumscribed social theory with inductive procedures and tentative conclusions, separating economics, psychology, and political science from positivistic quests for universal truth. The generalizations of nineteenth-century positivist theory had gone beyond reality to postulate an essential unity of knowledge and experience. The new social scientists, from 1880 to 1914, dismissed the grandiose ambitions of their predecessors and attempted instead to measure reality as it appeared in concrete problems, such as poverty, or trade, or county councils. Their methods and concepts introduced a behavioral social science for the analysis and solution of social problems. Marshall "recast" political economy "as the Science of Social Perfectibility,"¹¹¹ James tried to identify the psychological state of mind essential to social progress, and Wallas combined their perceptions into an "accurate science based upon research and not upon abstractions."¹¹²

None of the enthusiastic exponents of an inductive social science, including Wallas, suspected that a social scientist could select and arrange his data to serve arbitrary assumptions leading to unwarranted conclusions. Charles Kingsley, sickened by the conditions under which the majority of the population lived, had evoked "science" as the means of achieving the moral, spiritual, physical, and economic well-being of man.¹¹³ The new social scientists clung to this same faith in the inherent "goodness" of science, except that they made its effect conditional upon a precise application of method. Their only cautionary defense against the abuse of their methodology was to hope for a reverse Gresham's Law of thought in which insufficient ideas would eventually be driven away by more adequate concepts. The reforming ambitions of the new social scientists made them overlook the potential deficiencies in their method. They wanted to solve social problems by measuring their components, but, as recent social scientists have learned, the really important problems are often not measurable in the actual world in which they occur. Either the components are never entirely known, or, if they are taken into account, they may be so numerous as to be unmanageable. Another weakness of the new social scientists was the paucity of their data, too limited even for projects more modest than the complete reorganization of economic, social, and political institutions they proposed. The principles of individual and social behavior they inferred from their data were often naive and inaccurate. Yet, despite these deficiencies, the new social scientists understood that social theory, no matter how satisfying intellectually, had to be proven ultimately in practical experience.

Alfred Marshall, William James, and Graham Wallas began with an ethical

¹¹¹ *Pall Mall Gazette*, quoted in Keynes, "Alfred Marshall," 41.

¹¹² Harold Spender's review of Wallas' *Human Nature in Politics*, in the *Daily Chronicle*, Feb. 6, 1909. All the reviews, both negative and positive, mention this aspect of Wallas' work as the most novel. See also "Politics and Principles," *The Globe*, Dec. 30, 1908; J.[ohn] A.[dams], "Human Nature in Politics," *The Bookman*, XXXV (1909) 194-95; and reviews of *Human Nature in Politics*, *Manchester Guardian*, Dec. 14, 1908; and *Glasgow Herald*, Jan. 14, 1909.

¹¹³ Kingsley, "Science," in *Scientific Lectures and Essays* (London, 1885), 257.

commitment to social science that led them to project social sciences that were reforming in nature. They were convinced that values must be carefully scrutinized and rigorously stated to prevent theory and method from supporting special pleas for hidden or implicit schemes of value. But a social science implementing humanistic ends could not be relegated to a value-free realm of objectivity. Marshall, James, and Wallas were part of a greater revolution against those forces, neither understood nor anticipated, that threatened individual capacities for moral, reasoned choice. The new social sciences began as a systematic resistance to the undesirable inertia of history, and as a practical reforming program to improve the immediate conditions and qualities of contemporary life. From the late nineteenth century until World War I, social scientists were part of an active intellectual community of statesmen and men of letters, professionals and amateurs bound together by the belief that the contents and methods of science depended upon its purpose. It is a lesson that the experience of the generation following World War II may be teaching us again.

Germany and the Russo-Japanese War

JONATHAN STEINBERG

OVER thirty years ago, a French diplomat called his memoirs of the years 1904 to 1906 "Un grand tournant de la politique mondiale,"¹ and with some justice. Between January 1904 and the Algeiras Conference in the spring of 1906 war broke out between Russia and Japan, the first armed collision between two Great Powers in nearly thirty years; important new alliances were formed or broached; three major war scares erupted among the Great Powers in Europe; the Revolution of 1905 took place in Russia; and a large international conference of the Powers was convened. Recent research has helped to fill in the details of what was already a densely documented period.² Many questions, especially about French policy, have now been answered. Only the position of Germany, in spite of all the new material, remains puzzling. It is clear that between 1904 and 1906 Germany's diplomatic record was appalling. She alienated both belligerents—Russia and Japan, became involved in war scares with France and Great Britain, and forced on an unwilling world an international conference at which she found herself isolated. For all her hectic diplomacy, Germany earned nothing but suspicion. In August 1905 the future chief of the general staff, Helmuth von Moltke, noted in his diary that "all the other nations are pretty well united in reviling Germany and spreading the most terrible, stinking lies about us. . . . They all assert that we are disturbing the peace, and nobody seems to see that all Germany wants is to be left in peace."³ Even Baron Friedrich von Holstein, the chief architect of foreign policy from 1890 to 1906, admitted the bankruptcy of German diplomacy. "In short," he wrote to a friend, "in the present atmosphere, it seems to me that the correct and dignified thing to do would be to act like Russia after the Crimean War (*La Russie se recueille*) and calmly to withdraw into ourselves. . . ."⁴

A certain amount of German isolation was probably inevitable. In only one

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¹ Maurice Paléologue, *Un grand tournant de la politique mondiale 1904-1906* (Paris, 1934).

² Christopher Andrew, *Théophile Delcassé and the Making of Entente Cordiale* (London, 1968); Norman Rich, *Friedrich von Holstein: Politics and Diplomacy in the Era of Bismarck and William II* (Cambridge, 1965); J. A. White, *The Diplomacy of the Russo-Japanese War* (Princeton, 1964); George Monger, *The End of Isolation: British Foreign Policy 1900-1907* (London, 1963); Zara Steiner, "The Last Years of the Old Foreign Office, 1898-1905," *Historical Journal*, VI (1963), 59-90; Lamar J. R. Cecil, "Coal for the Fleet That Had to Die," *AHR*, LXIX (1964), 990-1005.

³ Moltke, diary entry for Aug. 22, 1905, in Helmuth von Moltke, *Erinnerungen, Briefe, Dokumente* (Stuttgart, 1922), 336.

⁴ Holstein to Otto Röse, June 18, 1906, *The Holstein Papers* (hereafter *HP*), ed. Norman Rich and M. H. Fisher (Cambridge, 1955-63), IV, no. 986, p. 429.

generation, Germany had grown to a position of Continental preponderance. The astounding economic expansion of Bismarck's Empire after 1871, the rapid growth of the German population, and the reputation of the Prussian military machine were bound to make the other powers increasingly apprehensive. After Bismarck's fall, as the consequences of Germany's strength became more evident, the Continental powers tended to look to alliances, such as the Franco-Russian Alliance of 1894, to redress the balance. Even Great Britain began to cast about for allies after 1899. This response was predictable; European diplomats had always made such calculations. What was not predictable was the German reaction. German diplomacy during the years before the First World War was so erratic and incalculable that it sometimes seemed as if the object of German statesmen were to hasten the Empire's isolation. German statesmen demanded that Germany be considered in every international crisis, even those in which German interests were trivial. Her diplomacy seemed restless and uncertain.

Norman Rich, in his work on Baron Friedrich von Holstein writes convincingly of the limitations of German diplomacy and its failure to distinguish between large and small issues or to construct a coherent set of priorities for foreign policy.⁵ The record of the Wilhelmstrasse was certainly unimpressive, but it is not the whole explanation. The diplomacy of Wilhelmine Germany reflected faithfully a system of government, and it was that system that failed, not only its diplomatic agencies.

The failure of the system became obvious during the First World War, but it also occurred, on a smaller scale, during the Russo-Japanese War. The irregular operations of Wilhelmine government in the years 1904 to 1906 accounted for a good deal of the ineptness of German diplomacy, although this was not apparent at the time. The myth of German efficiency blinded domestic and foreign observers to the fact that Germany was badly governed. The military monarchy presented an image of order and central control, of discipline and Prussian regularity; the reality was very different. During the Russo-Japanese War, precisely because it was wartime, some of the most unfortunate weaknesses of the German military monarchy were uncovered. These weaknesses in turn led to the sudden changes of course and random interventions abroad that created the impression of malevolence so deplored by General von Moltke. A crisis involving a risk of war brought out the worst in the government of the German Empire.

On the eve of the outbreak of war between Russia and Japan, Germany's diplomatic and military position had been deteriorating gradually, but not alarmingly, for three or four years. The Franco-Russian Alliance had been "reshaped" in 1899 and extended beyond the life of the Triple Alliance of Germany, Austria-Hungary, and Italy. France and Italy had arranged a settlement of colonial differences in November 1902, and there was a perceptible weakening of Italian

⁵ Rich, *Holstein*, II, 742-45.

enthusiasm for the Triple Alliance.⁶ Germany's relations with Britain were correct but a little strained. Misunderstandings in both capitals had led to an attempt in the years 1899 to 1901 to arrange an Anglo-German agreement, but each side believed that the other had been the suitor, and when the negotiations collapsed, each felt that the other had been less than candid. In German eyes, Britain had then rushed into an alliance with Japan, an alliance of the very sort she had refused to Germany, and was now, in the spring of 1903, moving alarmingly close to Germany's bitterest foe, the French Republic.⁷ The German foreign ministry had some grounds for concern about the deterioration of Germany's international position but some cause for optimism as well. Russia and Japan seemed to be heading for a military collision in the Far East. The British were tied to the Japanese, the French to the Russians, the Germans to neither. It was the classic Bismarckian position, that of *tertius gaudens*, in which Germany now found herself. In July 1903 the Russian ambassador called on Otto von Mühlberg, the deputy state secretary, to sound out the German position. Mühlberg got the distinct "impression that in the event of a conflict in the Far East we shall have to cope with formal Russian requests for help or support." What price to exact for such support was not yet clear. Bernhard Count Bülow, the German chancellor, noted in the margin: "We shall have to deal with this matter with the greatest care and delicacy. His Majesty hinted to Tsar Nicholas in Reval and Danzig that in the event of an attack on Russia in the Orient he would cover the Russian rear."⁸ On the other hand, as Bülow wrote to Holstein in January 1904, "from the point of view of our internal policies and to counteract the general dissatisfaction in Germany, it would of course be a good thing if 'somewhere far away' the nations came to blows."⁹

If war broke out between Russia and Japan, there would be immediate European complications. The Baltic and North Seas might become the scene of hostilities. In addition, Russian warships would have to pass through the narrow waterways between Denmark and Sweden. The Danish attitude toward the belligerents directly affected Germany, whose seacoasts and fleets were divided by the peninsula and islands of the kingdom of Denmark. In November 1903 the Kaiser told Admiral Alfred von Tirpitz, the state secretary of the Reichsmarineamt, that the issue of Danish neutrality had been raised in a discussion with the king of Denmark and the tsar. If Great Britain joined Japan in the war with Russia, how would Denmark react? Tirpitz recorded in his notes that the king of Denmark had been "crushed" by the thought of it.¹⁰

⁶ Andrew, *Delcassé*, 119-35, 138-46; E. N. Anderson, *The First Moroccan Crisis, 1904-06* (Chicago, 1930), 19-23.

⁷ The German ambassadors in St. Petersburg and Paris and the chargé d'affaires in London were asked to comment in May 1903 on the possibility of an Anglo-French-Russian understanding. See *Die grosse Politik der europäischen Kabinette 1871-1914, Sammlung der diplomatischen Akten des Auswärtigen Amtes* (hereafter GP), ed. Johannes Lepsius et al. (Berlin, 1923-27), XVII, no. 5370, pp. 570-72, and the replies, nos. 5371-73, pp. 573-80.

⁸ Bülow, marginal notes on Mühlberg, memorandum, July 15, 1903, GP, XIX, pt. i, no. 5924, pp. 10-12.

⁹ Bülow to Holstein, Jan. 16, 1904, HP, IV, no. 818, p. 277.

¹⁰ Tirpitz, notes, Nov. 14, 1903, in Tirpitz Family Papers (hereafter TFP), file entitled "Entwick-

The German government had a direct interest in Danish neutrality and was determined that the Danes should interpret that neutrality in an acceptable manner. The first point to clarify was the Danish attitude toward the provision of pilots for belligerent vessels. The ambassador to Denmark, Wilhelm von Schoen, reported to Berlin that

the Danish government believes that Denmark has a legal obligation to keep the Sound and the Belts open. When the dues were extinguished on the waterways [by the Convention of 1857], Denmark undertook to provide pilots, for if the Danish government failed to provide them for warships, one might easily go aground and thus block the narrow waterways.¹¹

But these narrow waterways were also vital to the deployment of the German battle fleet in the North and Baltic Seas. If Germany were at war with England, she could not permit the Danes to provide pilots for enemy shipping. Admiral Wilhelm Büchsel, the chief of the Admiralstab, put the matter bluntly:

The main thing is to find some good excuse for not observing Danish neutrality. We have to make certain that the Reich chancellor understands this and approves the navy's attitude. We have to make all the necessary preparations in peacetime. . . . Our minister in Copenhagen must also be clear that in the event of a war we shall regard the provision of pilots to our enemies as incompatible with Danish neutrality.¹²

Admiral von Tirpitz, state secretary of the Reichsmarineamt, did not agree with Admiral Büchsel, chief of the Admiralstab, and throughout the war the two competing naval administrations corresponded with increasing bad temper about the issue.¹³ It was a uniquely German state of affairs. Three separate naval administrations, all with direct access to the Kaiser, operated in intense competition: the Reichsmarineamt (imperial naval office) with ministerial responsibility and parliamentary obligations, the Admiralstab der Marine (admiralty staff) modeled on the position of the general staff as the organ by which the Kaiser's supreme command functions were to be carried out, and the Marinekabinett (imperial naval cabinet) responsible for personnel and the secretarial aspects of the Kaiser's command, but, in fact, by sheer proximity to the Kaiser, as influential as the two larger bodies.¹⁴

lung der Marine," VIII. The Tirpitz Family Papers, in the family's home at Irschenhausen, near Munich, Germany, were made available to me through the kindness of the late Korvetten-Kapitän a. D. Dr. Wolfgang von Tirpitz.

¹¹ Schoen to foreign ministry, Apr. 16, 1904, Akten des Admiralstabs der Marine, Abteilung A (hereafter CU-ADMA), II, Russland 11 b, no. 51. The copies of the German naval archives cited here and below, unless otherwise indicated, are located in the Seeley Historical Library, Cambridge University. Other abbreviations for collections of documents located here are as follows: for the Reichsmarineamt, CU-RMA; for the Marinekabinett, CU-MK.

¹² Büchsel, marginal notes on Schoen to foreign ministry, Feb. 25, 1905, and on internal memorandum, Mar. 23, 1905, CU-ADMA, II, Russland 11 b.

¹³ See Reichsmarineamt to Admiralstab, Apr. 17, 1905; draft reply Apr. 18, 1905; Friedrich von Ingenohl, *Seine Exzellenz vorzulegen*, May 12, 1905: "The RMA apparently thinks it can decide independently about our interests and the proper conduct of war in matters of international or maritime law . . ."; Admiralstab to Reichsmarineamt, May 17, 1905; Tirpitz to Büchsel, Jan. 6, 1906; all in *ibid.*

¹⁴ For the most complete account of this curious administrative arrangement, see Walther Hubatsch, *Der Admiralstab und die obersten Marine-Behörden in Deutschland, 1848-1945* (Frankfurt, 1958), 75-85.

The deployment of ships at sea clearly belonged to the Admiralstab. A German cruiser squadron was stationed in the Far East, and the Admiralstab was quite properly concerned about the prospects of war between Russia and Japan¹⁵—its possible consequences for Germany and for the naval balance. In January 1904 the Admiralstab alerted the German cruiser squadron in China and issued the following instructions: "While preserving strict neutrality, you are to observe the development of events in the north; in the Yangtse, observe the English. Report immediately any alteration in their position."¹⁶ The Admiralstab also wanted observers at Russian and Japanese headquarters when war broke out. On January 9, 1904, Admiral Freiherr von Senden Bibran, the chief of the Marinekabinett wrote to Count Bülow:

In order to obtain more rapid and reliable intelligence about the conduct of the war, in particular about the effects of modern weapons on modern targets, upon which important conclusions may well be drawn for our tactics, it would be of great value if German naval officers could be attached to the headquarters of the belligerent fleets. . . .¹⁷

Sealed orders were prepared and dispatched to Friedrich Count Alvensleben, German ambassador in St. Petersburg, and Emmerich Count Arco-Valley, German ambassador in Tokyo. If war broke out, Alvensleben and Arco were to approach the two belligerents and request that German naval observers be permitted to report to their headquarters.¹⁸

When news of the daring Japanese attack on Port Arthur on February 8, 1904, reached Berlin, the navy's plan went into operation. The results were excellent. For a long time Germany was the only power with observers inside Port Arthur and with ships on patrol in northern waters. These movements were duly noted by the other powers in China. As Vice-Admiral von Prittwitz, commander of the German cruiser squadron in the Far East, reported: "None of the other nations have sent warships to the theater of war. Our movements are being watched suspiciously by the English press."¹⁹ In August he warned the Admiralstab that "there is widespread distrust of Germany out here. Nobody believes that we shall be content with the leasing of Kiaochow and many think that we shall do in Shantung what the Russians have done in Manchuria."²⁰

The Japanese certainly shared these suspicions of Germany and they treated the German naval attaché in Tokyo with the utmost caution. In contrast, the Russians positively embarrassed Kapitän Paul Hintze, the attaché in St. Petersburg,

¹⁵ Admiralstab to Kaiser, "Hat der Beginn eines Krieges mit Russland im Laufe des Winters für Japan Vorteile," *zum Immediatvortrag*, Dec. 3, 1903, German naval archives in Militärgeschichtliches Forschungsamt-Dokumentenzone (hereafter MGFA-DZ), Freiburg im Breisgau, F. 2017, PG 65962.

¹⁶ Admiralstab to chief of the cruiser squadron, Jan. 7, 1904, CU-MK, XXIV. g.

¹⁷ Senden to Bülow, Jan. 9, 1904, *ibid.*

¹⁸ Senden to Büchse, *ganz geheim*, Jan. 9, 1904, *ibid.*; see also Bülow to Senden, Jan. 19, 1904; Alvensleben to foreign ministry, code telegram no. 66, Feb. 8, 1904; Arco to foreign ministry, Feb. 7, 1904; all in *ibid.*

¹⁹ Prittwitz to Admiralstab, telegram, Feb. 27, 1904, *ibid.*, vol. 1.

²⁰ Prittwitz to Admiralstab, telegram, Aug. 10, 1904, *ibid.*, vol. 2.

with confidences. Prince Orloff, the tsar's personal naval cabinet chief, even asked him to write confidential memoranda for His Majesty. "The tsar," Prince Orloff said, "wants to be as well informed as the Kaiser."²¹

Information about Russia's prospects, most of it alarming, poured into Berlin during 1904. As early as May of that year the able, Russian-speaking observer at Port Arthur, Korvetten-Kapitän Albert Hopman, reported on the impressive qualities of the Japanese navy:

Having seen these examples of a fanatical spirit of attack and endeavor, and comparing it with the apathy and indifference of the Russians, I can see only a black future. One can hardly imagine that the Japanese army has a different spirit. It has the same blood, the same nerves, as the navy.²²

The reports of the Admiralstab to the Kaiser of March 30 and April 20 included very unfavorable comments on the inefficiency and badly managed operations of the Russian squadrons, the low morale and poor quality of the commanders, and the dim prospects of the Russian army.²³ This information was important and ought to have affected German diplomacy. None of it got to the Wilhelmstrasse. By an order of February 10, 1904, renewed the following year, the Kaiser decreed that attachés and observers were to report "as do independent commanders of My ships overseas and the reports are to circulate among the military and naval authorities only."²⁴ Had more information about Russia's true military situation reached the chancellor and Holstein earlier, these men might have been more cautious in their dealings with Russian requests for aid.

Another odd feature of Germany's diplomacy in this early stage of the Russo-Japanese War is the government's failure to issue the customary declaration of neutrality. The terms of such a declaration were drafted in the foreign ministry and circulated to the relevant authorities on February 17, 1904, with the emphatic direction: "It is not proposed to make this declaration public." There is no obvious reason why such a decision should have been made. The actual terms of this unpublished declaration conformed to those of the published declarations of the other Great Powers. The Kaiser Wilhelm Kanal was to be closed to all belligerent vessels and German ports were to afford warships safe harbor for no more than twenty-four hours. Article 3 stated that "coaling of belligerent vessels was permissible within the terms of international law to that fixed amount that will permit passage to the nearest port of the belligerent power."²⁵ The decision not to publish this declaration was an unfortunate one. It allowed Albert Ballin, the general director of the Hamburg-Amerika Line, to arrange a large coaling contract with Russia, which led to one of the most difficult situations for German diplo-

²¹ Quoted in Hintze to Tirpitz, Dec. 21, 1904, MGFA-DZ, F. 2044, PG 66077.

²² Hopman to Admiralstab, May 4, 1904, CU-MK, XXIV. g., vol. 2.

²³ Admiralstab, *zum Immediatvortrag*, Mar. 30, 1904; *zum Immediatvortrag*, Apr. 20, 1904; both in *ibid.*, vol. 1.

²⁴ Orders of Feb. 10, 1904; Feb. 5, 1905, *ibid.*

²⁵ Foreign ministry to Reichsmarineamt, *ganz geheim*, Feb. 27, 1904, CU-ADMA, II, Russland 11 b, vol. 1.

macy during the entire period. It also gave the Admiralstab another occasion for interdepartmental agitation. The Admiralstab felt that the foreign ministry was neglecting matters of great importance. Precedents for a future war with England were being established by default. "If we continue to hesitate," the Admiralstab argued, "our area of maneuver in international customs and our choice among them will shrink further. English influence will be victorious everywhere."²⁶

The foreign ministry refused to support the Russian interpretation of prize and contraband, although the Russians, in the navy's view, were acting on the principles that a German government would be bound to adopt in a war with Britain.²⁷ As Holstein put it in a memorandum late in February 1904:

And what could possibly cause Germany to stand by Russia and endanger herself? . . . One can admire the bravery and other great qualities of the Russian Army and Navy. . . . But if Germany were to take sides, her world trade would be endangered. If one tickles the tiger, one must expect him to use his claws.²⁸

The Admiralstab found this caution exasperating and short-sighted. Its feelings were later summed up by Captain Friedrich Ingenohl in an angry minute: "In other words, we don't know what we want and we are supposed to be satisfied with this state of affairs. It is really impossible."²⁹

Ingenohl's criticism had a certain justification, for if the foreign ministry were right in adopting a cautious attitude toward questions of international law, it was unwise not to make that position clear to others. In August 1904 some Russian warships sought shelter in Kiaochow. The proper conduct of the port authorities ought to have been clear, given the terms of the unpublished declaration of February 17, but Kiaochow, although a colonial possession, was administered by the Reichsmarineamt—another oddity of the German governmental structure. The navy disliked the rule that the warships must be disarmed if they failed to leave port within twenty-four hours after the restoration of their seaworthiness. At first the governor of Kiaochow, a naval officer, refused to force the Russian warships to disarm, and when the Japanese ambassador in Berlin protested, a small international crisis ensued.³⁰ Eventually, the navy was compelled by the direct intervention of the Kaiser to accept the position of the foreign ministry that the ships be disarmed.³¹ The *Law Journal* in Britain commented:

The decision of the German government that the ships at Tsingtao must be disarmed is important as a recognition by a great naval power of a rule which before the present

²⁶ Admiralstab to Reichsmarineamt, *ganz geheim*, Nov. 5, 1904, *ibid.*, vol. 2.

²⁷ There is a voluminous correspondence between the Admiralstab and the Reichsmarineamt on these questions of international law, as well as drafts for presentation to the Kaiser, in *ibid.*

²⁸ Holstein, memorandum, Feb. 22, 1904, *HP*, IV, no. 823, p. 283.

²⁹ Ingenohl, minute on Tirpitz to Büchsel, *ganz geheim*, Jan. 13, 1906, CU-ADMA, II, Russland 11 b, vol. 3.

³⁰ Eickstedt to Hans von Plessen, telegrams, 12:46 A.M., 12:53 P.M., Aug. 13, 1904, CU-MK, XXIV. g., vol. 3; the Japanese ambassador to State Secretary Oswald von Richthofen, telegram, Aug. 13, 1904; Mühlberg to Eickstedt, telegram, Aug. 14, 1904; Eickstedt to Trüppel, telegram, 9:45 P.M., Aug. 14, 1904; all in CU-ADMA, II, Russland 11 b, vol. 2.

³¹ Wilhelm II to governor of Kiaochow, telegram, 3:50 P.M., Aug. 13, 1904, CU-MK, XXIV. g., vol. 3.

war could scarcely be said to have been universally accepted but which is founded on a sound conception of neutrality.³²

German relations with Japan were not improved by the incident, and British praise was little compensation. Admiral Büchsel remarked, "England's interests in these questions are opposed to ours."³³

Another persistent problem of the German government arose from the activities of military and naval attachés. In Prussian tradition, the Kaiser was supreme war lord. His relations with officers, in service or retired, was direct and personal; civilians could not intrude. Under Wilhelm II, who was inclined to high feudal concepts of loyalty, these relationships became a threat to the control of the German foreign ministry over external affairs. Writing in 1896, Holstein had warned Hugo von Radolin:

Internal conditions, especially at Court, are less pleasant. The Cabinets and Aides-de-Camp are becoming more and more an organised secondary government. The Aides-de-Camp have actually been assigned to various departments: Count Moltke, Austria and Germany; Colonel [illegible] Russia; Colonel Arnim, Alsace-Lorraine and England; Colonel Engelbrecht, Italy; Colonel Scheele, African Colonies, etc.³⁴

One of these influential officers was Colonel Oskar von Chelius, military attaché at the German embassy in Rome—"that ambassador of the future," as the official ambassador once called him.³⁵ In January 1904 the Kaiser instructed Chelius to prepare an itinerary for the annual Mediterranean cruise. Chelius carried out his instructions with diligence, arranging dates, secret service protection, and ports of call. The only item he neglected was to consult either the ambassador or the foreign ministry.³⁶ Such an omission would not have been serious under ordinary circumstances; the Kaiser's many journeys were usually arranged in a similarly casual manner,³⁷ but the spring of 1904 was a delicate time to arrange a visit to Italy. The French president, Émile Loubet, was expected for a major state visit April 24 to 28, the very time when the Chelius itinerary put the Kaiser's yacht in Italian waters. Should the reception given to the French president exceed in warmth that accorded to the Kaiser, in Bülow's view, "it would mean *le glas funèbre* of the German-Italian alliance."³⁸ A tense and thoroughly gratuitous crisis occurred in German-Italian relations. Germany exerted great pressure on Italy, but the Italians offered only weak concessions. President Loubet's visit was a tri-

³² Reprinted in London *Daily Graphic*, Aug. 19, 1904.

³³ Büchsel, *zum Immediatvortrag*, Aug. 27, 1904, CU-ADMA, II, Russland 11 b, vol. 1.

³⁴ Holstein to Radolin, Mar. 22, 1896, *HP*, III, no. 536, p. 601.

³⁵ Anton Count Monts to Holstein, Mar. 18, 1904, *ibid.*, IV, no. 825, p. 285.

³⁶ Chelius to Oberhof- und Hausmarschall, August Count zu Eulenburg, Feb. 5, 1904, CU-MK, XXXVI, vol. 22.

³⁷ The arrangements for this trip were so casual that the chief of the Admiralstab was unaware of the Kaiser's intention to take Germany's newest and most powerful cruiser as escort. See Büchsel to Senden, Mar. 9, 1904: "I have the honor to reply to Your Excellency's letter of the 7th of this month. In this letter I learned for the first time of the forthcoming journey of S.M.S. *Friedrich Karl*. I have immediately informed the foreign ministry." *Ibid.* There had been sixteen major pieces of correspondence during January and February about the cruise, but no one had informed the man responsible for deployment of the German battle fleet, or the agency responsible for making diplomatic preparations, that a large, modern warship would be making the cruise.

³⁸ Bülow to Monts, Mar. 6, 1904, *GP*, XX, pt. i, no. 6389, p. 40.

umph.³⁹ The Kaiser and the German foreign ministry were irked, and German-Italian relations deteriorated. On May 12 Holstein wrote that "to try to bring the Italians back from their change of course by reasonable arguments or threats is hopeless. The time is not appropriate for really effective intimidation."⁴⁰ None of this unpleasantness in relations between Germany and Italy would have occurred if, as the ambassador had suggested, the Kaiser's plans had been changed in time.⁴¹ Muddle, not design, had put the Italian government in a very awkward situation. After all, the Anglo-French colonial agreement, the foundation of the Entente Cordiale, had been signed on April 8. European statesmen were bound to see the Kaiser's presence in Italian waters during the visit of the French president as a warning to Italy and as a reaction to the Anglo-French treaty. Although the incident should not be exaggerated, it deepened suspicion where it had already taken root. The Foreign Office in London was only too ready to believe the worst of Germany, and several years later Eyre Crowe summed up this suspicion in his famous memorandum of January 1907:

Germany's policy always had been, and would be, to try to frustrate any coalition between two States which might result in damaging Germany's interests and prestige; and Germany would, if she thought such a coalition was being formed, even if its actual results had not yet been carried into practical effect, not hesitate to take such steps as she thought proper to break up the coalition.⁴²

A more significant, and genuinely devious, operation was the attempt to influence Russian war policy through the naval attaché in St. Petersburg, Kapitän Paul von Hintze. Behind this move was the growing uneasiness of the German navy about Russia's conduct of the war. In spite of the Kaiser's emphatic declaration—"That the Russians should lose! I don't even think of it in dreams,"⁴³ evidence continued to accumulate indicating that Russia would lose and lose badly.⁴⁴ August was a particularly bleak month for the Russians. On the tenth Admiral Witgeft's halfhearted attempt to run the Japanese blockade of Port Arthur failed miserably, and Russian naval prestige suffered a severe blow. On August 20 General Moresuke Nogi's Third Army began its land assault on Port Arthur.⁴⁵ In early September Bülow, who was on holiday in Norderney, sent a telegram to Friedrich Count Pourtalès, deputy state secretary, in Berlin:

When Admiral von Tirpitz returns from holiday, please raise with him—verbally and in strictest confidence—the issue of the Russian fleet. Find out whether our naval

³⁹ *Ibid.*, nos. 6391-419, pp. 42-78; *HP*, IV, nos. 824-27, pp. 284-88; Anderson, *First Moroccan Crisis*, 143-47.

⁴⁰ Holstein, memorandum, May 12, 1904, *GP*, XX, pt. i, no. 6416, p. 72.

⁴¹ Monts to Bülow, Apr. 6, 1904, *HP*, IV, no. 926, p. 288 n.3.

⁴² Crowe, memorandum, FO 371/257, Jan. 1, 1907, *British Documents on the Origins of the War 1898-1914*, ed. G. P. Gooch and Harold Temperley (London, 1927-38), III, app. A, p. 400.

⁴³ Wilhelm II, marginal note on Speck von Sternburg to foreign ministry, Aug. 31, 1904, *GP*, XIX, pt. ii, no. 6264, p. 537.

⁴⁴ Alvensleben to Bülow, Aug. 25, 1904, *ibid.*, pt. i, no. 6048, pp. 212-15. Hopman's report on the events of July and August, which arrived from Port Arthur on Sept. 2, 1904, was very damaging. For this and others, see CU-MK, XXIV, g., vols. 2, 3; reports from Trummel (Tokyo), Hintze (St. Petersburg), Hopman (Port Arthur), and Ritter von Gilgenheimb (Port Arthur), all in CU-ADMA. II, Russland 11 b, vols. 1-3.

⁴⁵ Christopher Martin, *The Russo-Japanese War* (London, 1967), 121-30.

attaché in St. Petersburg, Hintze, could make it clear to the Russians, entirely on his own and without arousing suspicion, that the present tactics of the ships locked up in Port Arthur will be disastrous for the Russian cause. . . . To you and to Excellency von Tirpitz I need hardly say how useful it would be for us if the war weakened Japanese naval strength as much as possible.⁴⁶

On September 15 Pourtalès called on Admiral von Tirpitz; later he reported to the chancellor:

Admiral von Tirpitz replied that he shared the opinion of your Excellency with regard to the tactics of the Russian warships in East Asia and *had already in the summer* given instructions to our naval attaché in St. Petersburg, directing him to express himself with the required caution in leading Russian naval circles in the manner suggested by your Excellency. Kapitän Hintze had *already* acted on this instruction and it appeared not entirely impossible that the recent emergence of the Russian fleet from Port Arthur could be attributed to this.⁴⁷

Such an undertaking was risky at best, but the fact that Tirpitz had decided to try it on his own is in some ways less revealing than the absence of surprise or at least comment by Count Pourtalès. By December 1904 Hintze himself became uneasy "in view of the great dangers in communications of this sort."⁴⁸ Tirpitz replied sharply:

With the authorization of the Reich chancellor in September of this year, I gave you certain unequivocal instructions in a given direction in which our interests lay and still lie. You replied to me by telegram on this and other matters on September 24. An official authorization for certain undertakings can never be given, and you must face the real possibility in such affairs of being disavowed later. That lies in the nature of the activity.⁴⁹

Once again a military authority had intervened in the conduct of foreign affairs. That the foreign ministry subsequently resorted to the same doubtful technique reinforces the general impression that the structure of the Kaiser's Germany encouraged, or at least condoned, such undertakings. Here too it can be argued that not much harm was done, that the intervention was trifling and, apart from the admiral's utterly unfounded boast, apparently ineffective. By itself it would have been nothing, but it was not by itself. It was part of a continuous pattern of independent naval and military activity abroad.

German business firms also had a tendency to conduct independent foreign policies, and in June 1904 the Hamburg-Amerika Line (HAPAG) signed a coal-ing agreement with an agent of the Russian government without consulting the German foreign ministry. The agreement, covering 338,000 tons of coal, obliged HAPAG to supply the needs of a Russian squadron under Admiral Zinovi Rozhdestvenski, which was to leave for the Far East in the autumn of 1904. The agree-

⁴⁶ Bülow to Pourtalès, Sept. 9, 1904, *GP*, XIX, pt. i, no. 6053, pp. 220–21.

⁴⁷ Pourtalès to Bülow, Sept. 15, 1904, *ibid.*, no. 6054, pp. 221–22 (italics mine); see also Tirpitz, note, handwritten, Sept. 26, 1904, MGFA-DZ, F. 2044, PG 66077. Both accounts of the meeting on September 15 are in substantial agreement.

⁴⁸ Hintze to Tirpitz, Dec. 21, 1904, MGFA-DZ, F. 2044, PG 66077.

⁴⁹ Tirpitz to Hintze, Dec. 27, 1904, *ibid.*

ment stated that the coaling would be carried out at points between the east coast of Denmark and the Chusan Archipelago, sixty miles south of Shanghai. On September 12, 1904, the London *Shipping Gazette* published an account of the contract, and when the Japanese ambassador in Berlin asked the foreign ministry for an official denial, Count Pourtalès replied that since the agreement was private and commercial, the German government could not intervene. In reality however, the German government was very much involved and concerned. If it forced HAPAG to cancel, it would antagonize the Russians and possibly frustrate the plans for the dispatch of the squadron. The German government could hardly do so while, at the very same moment, urging the Russian navy to be more aggressive. On the other hand, the Japanese might well regard the German colliers as belligerent vessels. By well-established custom they would be within their rights to halt or seize the HAPAG colliers, and a very nasty international incident might result. Bülow first thought that HAPAG should be forced to find "ways and means" to get out of its agreement, but Albert Ballin, the general director of the steamship company, was persuasive and a friend of the Kaiser. On September 23 Ballin persuaded Bülow to change his mind and let the contract stand.⁵⁰ The decision was probably the only one the chancellor could have made, but it involved grave risks and certainly compromised German neutrality for the private gain of a large firm.

German neutrality was compromised in other ways. Prince Henry of Prussia, the Kaiser's brother, returned to Germany in August 1904 from a visit with the tsar. He brought with him a request for further help for Rozhdestvenski's squadron, for which HAPAG was supplying coal. The tsar asked the Kaiser to afford protection against the dangers of a sudden ambush by Japanese torpedo boats in the Baltic or North Seas.⁵¹ As always in such matters, the Kaiser acted without consulting the foreign ministry and ordered the Reichsmarineamt and the ministry of the interior to look out for "suspicious Japanese with luggage."⁵² Harbor masters were alerted and naval units were put on patrols. Once again, Germany had made an important move in Russia's direction without either diplomatic preparation or compensation.

By September 1904 British observers began to suspect that Germany and Russia had at the very least arrived at a general agreement. Sir George Clarke, secretary of the Committee for Imperial Defence, wrote to A. J. Balfour, then prime minister, that "there is a very close understanding between Russia and Germany."⁵³ The British government had some reason to feel betrayed. The summer had seen the signing of an Anglo-German arbitration treaty on July 12 and a cordial visit by

⁵⁰ Cecil, "Coal for the Fleet," 993-95.

⁵¹ See Mühlberg to ministry of the interior, *geheim, eilt sehr*, Aug. 21, 1904; Otto von Diederichs to naval commands, Baltic and North Sea, *ganz geheim*, Aug. 23, 1904; both in CU-ADMA, II, Russland 11 b, vol. 2.

⁵² Wilhelm II to Reichsmarineamt, Aug. 28, 1904; Hintze to Reichsmarineamt, Aug. 29, 1904; Pourtalès to Reichsmarineamt, Sept. 10, 13, 1904; Eickstedt to foreign ministry, Sept. 13, 1904; all in *ibid.*

⁵³ Clarke to Balfour, Sept. 7, 1904, quoted in Monger, *End of Isolation*, 165.

King Edward VII to the "Kieler Woche."⁵⁴ When an article in the *Times* on September 14, entitled "Russia and Germany: A Far Eastern Understanding," suggested that there had been a change, the British ambassador in Berlin, Sir Frank Lascelles, wrote the Foreign Office that "His Excellency, Baron von Richthofen, could assure me that there was not one word of truth in the statement and that no sort of agreement had been entered into between the two countries."⁵⁵ For others in the Foreign Office, rumors of a Russo-German agreement fed an already profound suspicion of everything Germany did. A few, such as Sir Francis Bertie, the British ambassador to Paris, resented the slight improvement in Anglo-German relations during the summer. In June he wrote a letter to his friend Louis Mallet, assistant clerk at the Foreign Office: "Your letter of the 2nd breathes distrust of Germany and you are right. She has never done anything for us but bleed us. She is false and grasping and our real enemy commercially and politically."⁵⁶

Neither the coaling agreement nor the assistance promised the Russian fleet were the results of deceitful diplomacy. They simply happened in that manner peculiar to Imperial Germany. Yet both were events of considerable significance. Germany had drifted into an association with Russia without having received the slightest compensation in return. What such an association might mean became clear when, during the night of October 21, 1904, the Russian squadron passed through the Dogger Bank fishing area near Hull. The inexperienced Russian crews mistook some small British fishing trawlers for the mythical Japanese torpedo boats, fired on them, sank two, damaged two others, and killed several fishermen. The British, from the king down, were outraged, and the national excitement was so intense that war between Russia and Great Britain seemed imminent.⁵⁷ The tense situation involved the entire European concert. By the terms of the Franco-Russian Alliance, France would be compelled to assist Russia, her old ally, against Great Britain, her new ally. Germany had become involved with Russia and was increasingly regarded as her ally by the British. If Britain attacked Russia, she might also attack Germany and use the occasion to rid herself of German mercantile and naval competition. The German government found itself facing the threat of war for the sake of a power that had so far promised Germany nothing. Germany's leaders felt they must try to secure an alliance with Russia, and on October 27 the Kaiser broached the idea in a private letter to the tsar.⁵⁸ Nicholas, hard pressed by Russian reverses in the Far East and very shaken by the dangers of war with Britain, eagerly accepted the German offer.⁵⁹ On October 30, scarcely a week after the Dogger Bank incident, Bülow submitted the draft of a Russo-

⁵⁴ See Anderson, *First Moroccan Crisis*, 150-51.

⁵⁵ Lascelles to the Marquess of Lansdowne, Sept. 22, 1904, Foreign Office Confidential Print, in Seeley Historical Library, Cambridge University, vol. 8533, 219/20, p. 177.

⁵⁶ Quoted in Steiner, "The Last Years," 76.

⁵⁷ White, *Diplomacy of the Russo-Japanese War*, 179-83.

⁵⁸ Wilhelm II to Nicholas II, telegram, in English, Oct. 27, 1904, *GP*, XIX, pt. i, no. 6118, pp. 303-04.

⁵⁹ Nicholas II to Wilhelm II, telegram, in English, Oct. 29, 1904, *ibid.*, no. 6119, p. 305.

German defensive alliance to the Kaiser. Germany had, he observed, reached a moment of "world-historical significance."⁶⁰ But in reality the situation was more prosaic. Frightened by risks, which her own uncoordinated activities had helped to create, Germany tried by one daring, diplomatic stroke to alter a set of relationships that had taken years to develop. There was no plan, no preparation, nothing of the tenacious and careful procedures by which Théophile Delcassé had secured the agreement with England in April 1904. This was a foreign policy of impulse and over-reaction.

There was also some question about the value of the alliance. What could or would Russia do for Germany? Against whom? On October 31, Bülow called a meeting of his senior diplomatic and military advisers, one of the few times during these eventful months when anything like a coordinated policy was attempted. The results were not encouraging. According to the notes taken by Kapitän Adolf von Trotha, the chief of the central department of the Reichsmarineamt who accompanied Tirpitz, the meeting was not a success for the chancellor. The state secretary of the foreign ministry, Baron Oswald von Richthofen, opposed the idea of an alliance. Both military chiefs, Count Alfred von Schlieffen of the general staff and Admiral Tirpitz of the navy, were also opposed. Only Holstein, Bülow's main adviser, spoke in support.⁶¹ Tirpitz left the meeting a very worried man and the following day he wrote to Richthofen urging that the attempt be dropped:

As I said yesterday, not only is the military value of an alliance with Russia virtually nil for a war at sea, but in my view even for a land war it does not essentially count for much. For even in the most favorable case, if the Russians decide to give us a few army corps against France, it seems to me that the value of 100,000, or even 200,000, men in a war in which millions will confront each other will be small, possibly not even worth the difficulties for the functioning of our military apparatus that the addition of Russian elements will inevitably bring. . . . Finally, let us take the case of most immediate interest to us. Suppose that England declares war on us alone and that Russia then decides to fight on our side. The alliance between France and Russia with its provisions directed against us will hamper the freedom of our decisions against France, while the actual Russian help will be of no consequence. . . . If the alliance were known, the entire force of English public opinion would turn exclusively against us. . . .⁶²

Nothing could be more characteristic of Germany's conduct during the Russo-Japanese War than that the heads of Germany's armed services should reject an alliance whose proponents saw it as strengthening Germany's defenses. The whole project had the marks of hasty, ill-considered improvisation. Tirpitz's criticisms and anxieties were justified.

For the next forty-eight hours, the German government operated under the impression that war might yet occur.⁶³ On November 1 the Kaiser wrote to Bülow:

⁶⁰ Bülow to Wilhelm II, *ibid.*, no. 6120, p. 306.

⁶¹ Trotha, "Sitzung Beim Reichskanzler," Oct. 31, 1904, MGFA-DZ, F. 2044, PG 66077.

⁶² Tirpitz to Richthofen, Nov. 1, 1904, *ibid.*

⁶³ For an account of the naval aspects of the Russo-British crisis, see A. J. Marder, *From the Dreadnought to Scapa Flow* (London, 1961), I, 111. For other details of this dramatic period, see Richard Hough, *The Fleet that Had to Die* (New York, 1958), 46-60.

"This evening, tonight or early tomorrow, a terrible drama may well begin, the consequences of which are unforeseeable. God be gracious and spare us. I am very worried."⁶⁴ By November 3 news reached Berlin that the danger had passed.⁶⁵ The Royal Navy had been called off, and Admiral Rozhdestvenski's motley squadron was steaming southward unmolested.

During the next few weeks, the German government had another rude surprise. The agitation in Britain, which had reached boiling point about the Russians, now turned against Germany. There were many in Great Britain who would have agreed with the First Sea Lord, Admiral Sir John Fisher, who had written to his wife late in October: "Things look very serious. It's really the Germans behind it all. Peace seems assured tonight but one never knows, as that German Emperor is scheming all he knows to produce a war between us and Russia."⁶⁶ Paul Wolff Count Metternich, the German ambassador in London, confirmed that this view was widespread:

Germany is presented as the true enemy. The German Emperor is said to have the intention of waiting until his fleet is large enough to take on England. . . . Germany is supposed to have given the Russian government friendly signals about the dangers which threatened the Russian fleet when it emerged from the Baltic . . . in order to bring about an Anglo-Russian conflict.⁶⁷

On November 12 the *Army and Navy Gazette* openly advocated a preventive war. The naval attaché at the German embassy in London, Fregatten-Kapitän Carl Coerper, alarmed the Kaiser with his highly colored reports of British preparations for war. The reorganization of the Channel and Home Fleets seemed to underline his ominous predictions.⁶⁸ Early in December the crisis reached a peak. Admiral Felix Bendemann, commander of the German North Sea station, declared that it was a perfect moment for Great Britain to attack and destroy Germany's navy in a sudden punitive strike.⁶⁹ Admiral Büchsel agreed, and at an audience with the Kaiser on December 3, he presented his plan for war with Britain. The first step involved an unannounced invasion of Denmark and seizure of the vital Danish waterways.⁷⁰ The Kaiser approved the operations plan and ordered the general staff to alert the IX and X corps in North Germany for a strike against Denmark.⁷¹ Ships were recalled from overseas stations as unobtrusively as possible.⁷²

⁶⁴ Wilhelm II to Bülow, Nov. 1, 1904, *GP*, XIX, pt. i, no. 6110, p. 290.

⁶⁵ Monger, *End of Isolation*, 174-75; *GP*, XIX, pt. i, nos. 6122-24, pp. 309-12.

⁶⁶ Sir John Fisher to Lady Fisher, Oct. 28, 1904, quoted in Marder, *Dreadnought to Scapa Flow*, I, 111.

⁶⁷ Metternich to Bülow, private letter, Nov. 1, 1904, *GP*, XIX, pt. i, no. 6111, p. 292.

⁶⁸ Coerper to Reichsmarineamt, Nov. 17, 18, 19, 1904; *Daily Telegraph* article of Nov. 28, 1904, with the Kaiser's comments; Büchsel, memorandum, Dec. 4, 1904; all in CU-ADMA, England 1 a.

⁶⁹ Bendemann, "Gedanken über die augenblickliche kritische Lage," *ganz geheim*, Dec. 3, 1904, MGFA-DZ, F. 2044, PG 66077.

⁷⁰ Büchsel, *Immediatvortrag, ganz geheim—von Hand zu Hand*, Dec. 3, 1904, *ibid.*, F. 2017, PG 65964.

⁷¹ Büchsel, *zum Immediatvortrag, ganz geheim—von Hand zu Hand*, Jan. 31, 1905, *ibid.*, PG 65965.

⁷² Richthofen, memorandum, Nov. 30, 1904, *GP*, XIX, pt. ii, no. 6150, pp. 356-57. Tirpitz opposed the recall because it might further provoke Britain. See Holstein, memorandum, Dec. 1,

On December 5 Holstein wrote in a memorandum that "as a result of some secret opinions, which have come to my attention in the last few days, I now believe—which I never have before—in the possibility of a war with England, in which the attack would come from the English side."⁷³

Characteristically, during this flurry of military planning the generals and admirals failed to inform the chancellor of the Reich or the foreign ministry about operations that would have the most profound consequences for Germany's international relations. This oversight was finally rectified over two months later. On February 6 Freiherr von Senden Bibran wrote to the chancellor:

His Majesty the Emperor and King has taken the occasion of an audience of the chief of the Admiralstab der Marine to declare that the latter be directed to keep the Reich chancellor up to date on the general features of the proposed operations plans of the navy for war and that an All-Highest Expression of Will in this regard be transmitted to the Reich chancellor.⁷⁴

Bülow's reply is astounding. Aside from some minor changes in the wording of the draft of the order, he was only concerned lest news of orders "for the war" should become known:

Not only must the All-Highest Cabinet Order proposed to me remain strictly confidential but also all measures necessary must be taken so that nothing can become known of the issuance of His Majesty's navy with orders for war. The responsibility for His Majesty's foreign policy obliges me to demand this. In the given political situation it would lead in my view to incalculable catastrophes if such "operations plans of the navy for war" were not to be treated with the greatest possible secrecy on everyone's part. . . .⁷⁵

When reflecting on this episode of German history, it is difficult to decide whether the conduct of the military planners or of civilian statesmen is more extraordinary. The military calmly set about the violation of Denmark's neutrality as if it were the autumn general staff war games. There was apparently no thought for the catastrophic consequences that such an action would have on world opinion or on the other Great Powers, let alone on the Kaiser's relations with his "Onkel Hans." The statesman responsible, as he himself emphasized, for His Majesty's foreign policy, never uttered a word on the issue itself, but merely noted that it would be extremely dangerous if such information became known. This is the very stuff of which Imperial Germany's ultimate disaster was woven.

In a different part of the Imperial government another sort of crisis began to develop at the same time. The threat of war put Admiral von Tirpitz in a very great dilemma. In 1898 and 1900 two substantial navy laws had been passed by the

1904; Klehmet, memorandum, Dec. 4, 1904; both in *ibid.*, nos. 6151, 6152, pp. 357–58. Under pressure, the Admiralstab modified its plans to recall ships from the China and West Indian stations. See Dick to Büchsel, Dec. 8, 1904, CU-ADMA, II, England 11 a.

⁷³ Holstein, memorandum, Dec. 5, 1904, *GP*, XIX, pt. ii, no. 6153, p. 359.

⁷⁴ Senden to Bülow, *ganz geheim*, handwritten, Feb. 6, 1905, CU-MK, XXXII, vol. 2; see also accompanying draft of A.K.O. (All-Highest Cabinet Order), Feb. 3, 1905, *ibid.*

⁷⁵ Bülow to Senden, handwritten, Feb. 9, 1905, *ibid.*

Reichstag. In consequence the German navy was expanding annually at the rate of two battleships, one heavy cruiser, two light cruisers, and two torpedo-boat divisions, although most of the planned expansion was still on paper.⁷⁶ Completion of the naval building program, as amended by the second *Flottengesetz* of 1900, might be expected in 1917, but the war was expected now. Sir John Fisher's redistribution of the British fleets assured Britain an immediate superiority in battleships alone of more than two to one.⁷⁷ Tirpitz had always argued in his "risk theory" that there would be a so-called "danger zone" between the point at which the growth of the German fleet came to the attention of the Royal Navy and the point at which its growth would assure it genuine deterrent power, that is, the point at which the risk to the Royal Navy's command of the sea would become just great enough to cause the Admiralty to exercise caution in considering a battle with the German navy. The "danger zone" could be traversed by a calm, restrained policy and a sound set of defensive alliances. But suddenly the theoretical danger had become actual, and an emergency meeting was held on December 8 at the Reichsmarineamt to discuss these questions.⁷⁸ A few days later, Kapitän August von Heeringen, Tirpitz's choice as his successor and perhaps his most trusted colleague, submitted a memorandum questioning the whole conception on which Tirpitz's plans rested:

The expansion of our fleet so far has been carried out on the assumption that we should be able to keep the peace until it had been completed. It was a reasonable assumption. . . . In the meantime, as a result of the Russo-Japanese War, all the powers have moved much nearer to the idea of war. . . . One of our obvious enemies has already drawn its sword from its sheath and is therefore an incalculable factor. . . . We simply cannot spend 200 millions a year for the navy and in this threatening time only have a battle-ready fleet for a few months of the year. . . . In every measure we take, we must ask ourselves not what will happen in the distant future, but what real increase in power it brings us in the time immediately ahead.⁷⁹

Tirpitz's reply underlines perfectly the dilemma of German naval expansion:

Since 1898 we have done our utmost to drive the fleet program forward; the navy laws, the highest possible navy estimates, postponement of coastal fortifications, curtailment of our overseas service, limitation on the number of gunnery practice ships, torpedo, training, and experimental ships, etc., etc., all prove this. The concentration of our resources on one purpose has been fought through against the resistance of the entire navy. That the program is not finished can only be used against us in a war if it can be shown that in the years 1898 to 1904 more could have been achieved. . . . The idea that we must subordinate "tomorrow" to "today" is correct only with the greatest reservations. The danger zone for Germany is not just there today, but will in all probability also be there tomorrow, and we must reckon with these facts in the development of our navy.⁸⁰

Tirpitz accused Heeringen of claiming that the navy laws were wrong because they were not completed, but that was not the point. The navy's difficulties arose

⁷⁶ Tirpitz, *Politische Dokumente* (Stuttgart, 1924), I, 13–20, app.

⁷⁷ Marder, *Dreadnought to Scapa Flow*, I, 42–46.

⁷⁸ "Protokoll über die Sitzung am 8. Dezember 1904," MGFA-DZ, F. 2046, PG 66086.

⁷⁹ Heeringen, "Schlussvotum des Vorsitzenden der R. Kommission," *ganz geheim*, Dec. 14, 1904, *ibid.* F. 2044, PG 66077.

⁸⁰ Tirpitz, handwritten note on Heeringen, memorandum, Dec. 20, 1904, *ibid.*

not because the laws were not yet carried out but because the danger zone was theoretically infinite. Tirpitz argued that Germany had to build a navy to deter an attack by Great Britain, the principal naval power, but as long as that power continued to expand its own fleet, the gap between the two competitors, and thus the danger zone, would remain forever. If there were no need to deter the principal naval power, there was no danger zone, but no need for a great fleet either. A crisis with Great Britain would always find the German navy incomplete.

The problem had a second aspect. When Tirpitz took over as state secretary of the Reichsmarineamt in June 1897 he had justified the concentration of Germany's navy at home by pointing to its political potential.⁸¹ A great fleet in the North Sea would make Germany a more attractive ally to the Continental powers, or would compel Britain to be more responsive to German demands. Events had reversed Tirpitz's predictions. German naval expansion had forced Britain to abandon her isolation and to seek allies of her own. In addition, the war in the Far East was rapidly reducing Russia, one of the Great Powers for whom the fleet's *Bündnisfähigkeit* had been designed, to a state of impotence, and was pushing France closer to Britain. The "risk theory" had produced an arms race and a vicious circle in German foreign policy as well.

The logic of the Tirpitz program demanded an alliance with Russia, and hopefully France, but when such an alliance seemed possible, Tirpitz rejected it, for in a crisis with England it would increase the risk to Germany. Thus even the foreign political corollary of the "risk theory" involved a vicious circle. The fleet increased English hostility; that hostility increased the need for an alliance against England; the alliance with Russia against England would offer no naval protection, it would merely increase the risk to Germany. Thus the fleet made alliances with both England and Russia impossible, and there was nothing left for Germany but diplomatic isolation.

By December it had become clear that Russia was unwilling to enter an alliance with Germany. On December 7, 1904, the tsar informed the Kaiser that the French, his allies of ten years' standing, would have to be consulted first on the terms of the treaty.⁸² The Kaiser and Bülow drew back. In view of the recent Anglo-French entente, they were certain that France would inform Britain at once of the proposed Russo-German treaty, and British hostility would be further increased. On December 13 Bülow recognized the point Tirpitz had argued on October 31:

The decisive point is as always whether an agreement, alliance or treaty of any kind with Russia would increase or decrease the danger threatening us from England. . . . While an agreement with Russia safeguarding the peace and raising our position in

⁸¹ For the complete text in German and English of Tirpitz's initial memorandum, "Allgemeine Gesichtspunkte bei der Feststellung unserer Flotte nach Schiffsklassen und Schiffstypen," see my *Yesterday's Deterrent: Tirpitz and the Birth of the German Battle Fleet* (New York, 1966), 208–23.

⁸² Nicholas II to Wilhelm II, Dec. 7, 1904; Wilhelm II to Nicholas II, Dec. 12, 1904; Bülow to Alvensleben, Dec. 12, 1904; Alvensleben to foreign ministry, Dec. 13, 1904; all in *GP*, XIX, pt. i, nos. 6130, 6131, 6135–38, pp. 322–31. See also Radolin to Holstein, Dec. 8, 1904, *HP*, IV, no. 866, p. 315.

the world would be a great success for our foreign policy and would be welcomed in wide and in the best circles as a return to the tradition of Bismarckian policy, a bond with Russia which would in contrast to this draw England's hostility upon us would certainly be condemned unanimously by the whole nation, by the German princes first of all.⁸³

On January 2, 1905, State Secretary Richthofen told Tirpitz about the Russian answer to the alliance proposal. Tirpitz replied: "On our side greater coolness is now appropriate. On the other hand it is still in our interest that the Russian fleet do some damage to the Japanese. I say this in view of the verbal instructions to Hintze."⁸⁴ Two days later Tirpitz went for a walk with the chancellor. He found Bülow in a bitter mood. The chancellor blamed the Kaiser for antagonizing all of Europe's sovereigns and even the German princes.⁸⁵ In the meantime, Hintze had become nervous again:

When I got back from the South, I found that our embassy here was working for peace or, at least, for the recall of the II squadron. I was told that the coaling contract was too risky and might mean war. These policies were in conflict with my instructions, and I am therefore writing for new orders.⁸⁶

Tirpitz hastened to reassure him:

The directives that I gave you verbally, according to instructions from the Reich chancellor, were not concerned with the question of peace or war. They reckoned only with the fact that a state of war existed and that certain activities of war best corresponded to our interests. The question of peace or war is a political one and outside my competence.⁸⁷

Once again a German military intervention was to have serious consequences. By the winter of 1904 the Russo-Japanese War had become desperately unpopular in Russia. In November, Count Sergei Witte, who had opposed many of the policies that led to war, had warned the German banker Franz von Mendelssohn: "Anyone who urges us to continue this war is no true friend of Russia."⁸⁸ Yet Tirpitz and his attaché in St. Petersburg were doing just that. No line could be drawn between politics and military matters, as Tirpitz suggested. To urge the Russians to "damage" the Japanese fleet in Germany's interest, to encourage the suicidal undertaking of the second Pacific squadron, and to risk international complications as a result of the coaling agreement, were more than purely military matters. In any case, whatever good will Germany may have gained by such military intervention was nullified by Albert Ballin's sudden decision to try to get out of the coaling agreement.⁸⁹ Throughout January, Ballin tried to get the foreign ministry and the Reichsmarineamt to agree to new arrangements. The Kaiser, thoroughly alarmed by the dangers of an international incident, told Ballin on February 1, 1905, that, without letting the Russians know, he was to make sure

⁸³ Bülow to Holstein, Dec. 13, 1904, *HP*, IV, no. 867, p. 316.

⁸⁴ Tirpitz, notes, in pencil, Jan. 2, 1905, TFP, "Entwicklung der Marine," VIII, bk. 2.

⁸⁵ Tirpitz, notes, Jan. 4, 1905, *ibid.*

⁸⁶ Hintze to Tirpitz, Jan. 11, 1905, MGFA-DZ, F. 2044, PG 66077.

⁸⁷ Tirpitz to Hintze, Jan. 23, 1905, *ibid.*

⁸⁸ Quoted in Bülow, memorandum, Nov. 2, 1904, *GP*, XIX, pt. ii, no. 6167, p. 388.

⁸⁹ Metternich to Bülow, Dec. 26, 1904, in Cecil, "Coal for the Fleet," 1002.

that no German ships, men, or flags proceeded beyond Madagascar.⁹⁰ The tsar wired the Kaiser and demanded that Ballin be compelled to honor his agreement.⁹¹ At Bülow's request, the Kaiser withdrew his instruction to Ballin and informed the tsar that the coaling arrangement was a private matter and that Ballin was "at liberty to act as he thinks fit of course at his own risk."⁹²

On January 22 tsarist guards had fired on a peaceful demonstration outside the Winter Palace, and by February the Russian Empire appeared to be facing a genuinely revolutionary situation. Germany was left with little to show for her efforts to support Russia. The negotiations for alliance had failed. The Russians were losing the war badly, and it was not impossible that Imperial Russia might cease to be an effective Great Power for some time to come. It was not surprising that Bülow and Holstein began to look toward an improvement in relations with Great Britain. But here the naval building program of Admiral von Tirpitz was the chief obstacle. In the middle of December however, the German government had received some encouraging news. Count Metternich, the German ambassador to London, returned for consultations and presented a long memorandum to the chancellor in which he assured his government that England had absolutely no intention of attacking Germany. "England wants no war; with anybody. It wants quiet and to recover from the financial consequences of the Boer War."⁹³ Encouraged by Metternich's views, Bülow decided to try to restore something like good relations with Britain. He invited the British ambassador, Sir Frank Lascelles, to the chancellor's palace on Christmas Eve 1904.

Bülow opened the conversation by pressing the point that the Germans really expected an English attack.⁹⁴ Lascelles replied that the British could see no justification for the German fleet except as a force to attack Britain.⁹⁵ Bülow decided to apply a little judicious blackmail. With satisfaction he wrote to Holstein:

My remark that we still did not have an alliance with Russia though our relations were excellent . . . made a great impression on L., though I had thrown it out lightly. Useful though my hint was, I still believe that we should not drill too deep in this direction, nor slap on the colors too heavily.⁹⁶

Bülow had miscalculated badly. "A great impression" had been made, but not the one he imagined. Austen Chamberlain was furious:

I think it is time that we spoke with equal frankness. When has German diplomacy ever done otherwise than 'lean to Russia'? In what question, where the interests of England and Russia conflict, have we had, or can we ever expect, the support of German

⁹⁰ Mühlberg to Heinrich von Tschirschky, Feb. 2, 1905, in *ibid.*, 1004.

⁹¹ Nicholas II to Wilhelm II, Feb. 14, 1905, *GP*, XIX, pt. i, no. 6092, p. 27.

⁹² Wilhelm II to Nicholas II, Feb. 15, 1905, *ibid.*, no. 6094, p. 272; Bülow to Wilhelm II, Feb. 14, 1905, *ibid.*, no. 6093, pp. 271-72.

⁹³ Metternich, memorandum, Dec. 18, 1904, *ibid.*, no. 6140, pp. 332-40; also reports of military attaché Major Friedrich Count von der Schulenburg, Dec. 14, 1904; third secretary of London embassy, Count Eulenburg, Dec. 15, 1904; all in *ibid.*, pt. ii, nos. 6154, 6155, pp. 359-67.

⁹⁴ Lascelles to Landsdowne, Dec. 25, 1904, telegram, secret, FO Print, vol. 8533, no. 286, p. 266.

⁹⁵ Wilhelm II, minute on Bülow to Wilhelm II, Dec. 26, 1904, *GP*, XIX, pt. ii, no. 6157, p. 374.

⁹⁶ Bülow to Holstein, Dec. 25, 1904, *HP*, IV, no. 873, pp. 321-22. The *GP* account of the Lascelles-Bülow meeting omits any mention of this phase of the conversation. XIX, pt. ii, no. 6157, pp. 372-74.

diplomacy? . . . Whatever Count Bülow may say, [Germany's] navy is a standing menace to this country. It has been openly recommended to German patriotism in the German press for this reason. . . .⁹⁷

Anglo-German relations continued to deteriorate. The voices of those in Great Britain who pleaded for conciliation grew fainter. On January 3, 1905, Sir Thomas Sanderson, permanent undersecretary at the Foreign Office, wrote to Lascelles in Berlin:

I wish we could make the lunatics here who denounce Germany in such unmeasured terms and howl for an agreement with Russia understand that the natural effect is to drive Germany into the Russian camp and encourage the Russians to believe that they can get all they want at our expense and without coming to any agreement with us.⁹⁸

The Bülow move was not without support in Britain. It might have been possible to do something to improve Anglo-German relations, had it not been for an unfortunate speech by Arthur Lee, civil lord of the Admiralty, on February 2. "If war should unhappily be declared," he stated, "under existing conditions, the British Navy would get its blow in first, before the other side had time even to read in the papers that war had been declared."⁹⁹ A minister of the crown had openly threatened Germany with a surprise attack like that on Copenhagen in 1807. The Kaiser wrote to Tirpitz:

Dear Maestro, You will have read in Wolff the astounding speech of the civil lord of the Admiralty with its open threat of war against us. I have just had Lascelles here and made it clear to him in unambiguous terms that this revenge-breathing corsair must be disavowed and rectified by his government by morning. Otherwise there will break out such a storm in our press that it can only be met through the speedy introduction of a colossal new building program, forced on us by "public opinion."¹⁰⁰

The next day Admiral Georg von Müller called on Tirpitz to discuss "more rapid creation of a stronger fleet."¹⁰¹ On February 10 Tirpitz was told by the chancellor that "the government would approve any sum he wanted, that the state secretary of the treasury was not alarmed by the sums, etc."¹⁰² On February 11 Tirpitz had an audience with the Kaiser to discuss what sort of addition to the navy law of 1900 ought to be introduced in the Reichstag. To Tirpitz's surprise and relief, the Kaiser was prepared to accept the very modest addition of the six cruisers struck by the Reichstag from the previous law. The Kaiser said: "I prefer this. I want no bill which has a politically dangerous point directed against England."¹⁰³ A more dramatic and possibly dangerous gesture was avoided, but only because the Kaiser and his chiefs of staff were still reckoning with the very real possibility of an English attack.¹⁰⁴

⁹⁷ Chamberlain, memorandum, Jan. 14, 1905, quoted in Monger, *End of Isolation*, 177.

⁹⁸ Sanderson to Lascelles, Jan. 3, 1905, quoted in *ibid.*

⁹⁹ Quoted in Anderson, *First Moroccan Crisis*, 181.

¹⁰⁰ Wilhelm II to Tirpitz, Feb. 4, 1905, MGFA-DZ, F. 2044, PG 66077; reprinted in Tirpitz, *Politische Dokumente*, I, 14.

¹⁰¹ Müller to Tirpitz, *ganz vertraulich*, Feb. 8, 1905, Tirpitz, *Politische Dokumente*, I, 14.

¹⁰² Tirpitz, Feb. 10, 1905, *ibid.*, 17. I have had occasion to compare the documents cited by Tirpitz with the originals and have found them, without exception, to be accurate and usually complete.

¹⁰³ Wilhelm II, minute, Feb. 11, 1905, *ibid.*, I, 18.

¹⁰⁴ A.K.O., Feb. 3, 1905, CU-MK, XXXII, vol. 2. See also "Denkschrift zum Immediatvortrag

A year had now passed since the outbreak of the Russo-Japanese War, and Germany's international position had gravely deteriorated. There were strained relations with Russia, in whose interests Germany had compromised her neutrality and run the risk of armed conflict with Japan through the coaling agreement. Relations with England had become so bad that war was now openly discussed. Relations with Italy had been weakened, and the French, the permanent enemy in German eyes, were now immeasurably stronger through the conclusion of the Anglo-French agreement of 1904. It was not a happy prospect and worse was to come, for the French seemed determined to gain a protectorate over the turbulent kingdom of Morocco without even consulting Germany. In this year of setbacks, the uncoordinated and restless activity of the German governmental authorities had unquestionably contributed to Germany's difficulties.

It was in this difficult situation that Bülow and Holstein began to pursue the policies that led to the Moroccan Crisis of 1905 and that brought Europe to the edge of war. Ironically, during this period civilian leadership entirely dominated both the court and the military. The precise objective of the policies of Bülow and Holstein has been much disputed. It may be, as some historians believe, that they hoped to provoke France into war while her Russian ally was unable to help.¹⁰⁵ They certainly intended to demonstrate to the French that the British alliance could not protect France in a land war. Whatever their motives, they succeeded for a time in giving the semblance of central control to German foreign policy, even if the policy eventually led to an international humiliation at the Algeiras Conference. Yet even during this period, civilian leadership had to cope with periodic interventions by the Kaiser and the independence of military planning.

There have been many theories about the causes of Germany's eccentric behavior before 1914, ranging from Erich Eyck's work on the "personal regime" of Wilhelm II to Thorstein Veblen's analysis of the incompatibility of modern technology and the dynastic state.¹⁰⁶ One simple possibility has often been ignored. As Friedrich Stampfer put it, "when the World War began, Germany was economically

über den Aufmarsch und die Verwendung S. M. Flotte im Krieg gegen England im Mobilmachungs-jahre 1905," *ganz geheim—von Hand zu Hand*, Mar. 21, 1905, MGFA-DZ, F. 2017, PG 65965.

¹⁰⁵ There is much circumstantial evidence to suggest that a decision to take a large risk over Morocco was made by Bülow and Holstein. On February 14, 1905, the chief of the general staff informed Büchsel that the troops required to carry out the occupation of Denmark in the original version of "O.P." (the "Operations Plans") (see p. 1978, n. 70) would not now be available "because in a war with England and France the necessary troops for the occupation of Zeeland could not be spared without endangering the chances against France." Büchsel, *zum Immediatvortrag*, Feb. 14, 1905, *ibid.* We know too from a letter of Holstein to his cousin that Schlieffen was a regular visitor at Holstein's office. See Helmuth Rogge, *Friedrich von Holstein: Lebensbekenntnis in Briefen an eine Frau* (Berlin, 1932), 155–56, 187. Norman Rich has assembled an impressive case, based on his detailed studies of the Holstein Papers, for the possibility of a preventive war. *Holstein*, II, 696–98. See also Gordon A. Craig, *The Politics of The Prussian Army* (Oxford, 1955), 283; Gerhard Ritter, *The Schlieffen Plan: Critique of a Myth* (London, 1958), 128; *Idem*, *Staatskunst und Kriegshandwerk*, II (Munich, 1960), 133; Peter Rassow, "Schlieffen und Holstein," *Historische Zeitschrift*, CLXXIII (1952), 301–20.

¹⁰⁶ Erich Eyck, *Das persönliche Regiment Wilhelms II* (Zurich, 1948); Thorstein Veblen, *Imperial Germany and the Industrial Revolution* (New York, 1915).

the strongest, best administered, and worst governed state in Europe."¹⁰⁷ It is easy to understand why such an argument has not found favor. Germany has for so long been clothed in a mythical aura of efficiency and discipline that the very idea of the Imperial government not functioning well has been dismissed. Yet shrewd observers at the time noticed chaos at the top, and they were right.

The Kaiser's personality has also tended to cover the weakness of the government structure. His flamboyance, his interventions, his mercurial disposition, and his lack of restraint certainly made a bad situation worse, but during the Russo-Japanese War and the First Moroccan Crisis, the Kaiser played a peripheral part in affairs. The confusions of German policy simply accumulated and cannot be blamed on any one individual, no matter how exalted. They arose from the inadequacies of an eighteenth-century structure of rule that maintained the fiction of monarchical absolutism long after such a system could be effectively maintained. The traditions of Frederick the Great and the continued independence of the military were inappropriate in the management of a modern industrial, bureaucratic state. The result was a paradox of the utmost efficiency in the levels below and a vacuum at the top. A Hobbesian war of all against all among overlapping authorities, independent military commands, and powerful personalities at court took place. Had the Kaiser been as modest as his grandfather these problems would have arisen.

The implications of the Frederician tradition were not confined to the powers of supreme command. They led to the separation of command from administrative and personnel functions in the armed services, so that the army and navy had three equally placed chiefs struggling for influence on royal decisions. The Prussian traditions subordinated civilian to military leaders in prestige, if not always in practice, and prevented the link between war and diplomacy that had been Bismarck's central concern. A structure of government that could be operated only by a genius, and even under him imperfectly, must have fundamental flaws.

During the Russo-Japanese War, a preview of things to come took place. There was little coordination but much activity, many plans but no central direction. The resulting muddle and uncertainty created the impression of menace and design in the minds of foreign observers, which only added to the store of ill will which a Great Power accumulated by its very greatness. During the First World War the failure of central control led to disaster and to military dictatorship as the one form of rule that could coordinate the Imperial regime. Ironically the Frederician legacy, expressed in the search for the genius-statesman, the one central figure who could command all, was strengthened in the process. In the calamitous history of Germany in the twentieth century, her conduct during the Russo-Japanese War stands out as an unfortunate but entirely characteristic episode.

¹⁰⁷ Quoted in Theodor Eschenburg, *Die improvisierte Demokratie. Gesammelte Aufsätze zur Weimarer Republik* (Munich, 1963), 20.

* * * *Review Article* * * *

World War II: A Survey of Recent Writings

LOUIS MORTON

TITLES OF THE BOOKS UNDER REVIEW ARE IDENTIFIED IN THE BODY OF THE ARTICLE BY ASTERISKS; COMPLETE BIBLIOGRAPHICAL INFORMATION FOR THOSE BOOKS APPEARS IN A LIST AT THE END OF THE ARTICLE.

If the First World War was, as Geoffrey Barraclough has observed, the great turning point of modern history, the Second World War looms as the decisive event of our own time, marking the end of one era and the beginning of another. Even with the recent revival of scholarly interest in the events of 1914-18, the literature on the First World War cannot compare in sheer bulk and variety with the vast outpourings on World War II. And there is no sign that the flood is abating: a fifty-page bibliography of selected writings on World War II prepared only a few years ago already needs to be brought up to date.¹ The present survey is an effort to do so, but only partially, for it includes only those volumes sent to the author for review, a fraction of the books on World War II published during the past year.²

The demand for books on World War II seems insatiable; the supply inexhaustible. World War II was a big war, fought on four continents and in every kind of terrain; it offers an almost unlimited supply of subjects for the enterprising writer, from the budding Ph.D. candidate in search of a thesis topic to the professional writer hoping for screen credits and the best-seller list.³ And it left a residue of controversies that continues to exert a particular fascination for historians. Nor is there, apparently, any lack of readers, despite the protest

► *Louis Morton, Daniel Webster Professor and chairman of the history department at Dartmouth, is editor of the US Army twelve-volume official history of the war against Japan and author of two of the publications in that series: The Fall of the Philippines (Washington, 1953) and Strategy and Command: The First Two Years (Washington, 1962). Mr. Morton, a specialist in the military and diplomatic history of the United States, received the doctorate from Duke University in 1938; he studied with William K. Boyd and Richard H. Shryock.*

¹ Louis Morton, *Writings on World War II*, American Historical Association pamphlet no. 66 (Washington, 1967).

² A bibliography of works in English on World War II published in 1969 listed over 900 titles, although the relation of some of these to the war is rather remote. American Committee on the History of the Second World War, *Newsletter*, no. 3, Dec. 1969. The best listings of books on the war are found in *Jahresbibliographie der Bibliothek für Zeitgeschichte* and the *Révue d'Histoire de la Deuxième Guerre Mondiale*.

³ A recent compilation of "Dissertations in American Military History, 1946-1968," by Allan R. Millet, listed about 350 titles of which 35 were on World War II. Unpublished, in possession of author.

against all matters military on campuses across the country and the low esteem in which the military profession is held. Whatever the reasons, military writing continues to enjoy wide popularity. Scarcely a week goes by without the publication of one or more books on the subject of war, and of these, the largest number are on World War II.

The events leading to the outbreak of hostilities in Europe in September 1939 have been documented and recorded in such overwhelming detail in official records, memoirs, biographies, and historical studies that there would seem to be little left for the historian to do. This is not the case, however, for the passage of time casts men and events in a different light, and old interpretations give way to new. Leonard Mosley's *On Borrowed Time* tends to support this generalization.* It takes the reader down the well-traveled path from Munich to the invasion of Poland, examining the major signposts along the way and introducing him again to the familiar cast of characters in the tragic story that raised the curtain on World War II—Chamberlain, Churchill, Daladier, Hitler, Ribbentrop, Stalin, Molotov, Benes, Masaryk, Beck, Roosevelt, Joseph Kennedy, and Lindbergh.

On Borrowed Time holds no surprises for the student of the prewar period and offers little of consequence that is new. Its thesis, by now a familiar one, was most effectively stated by A. J. P. Taylor in *The Origins of the Second World War* (London, 1961): War came to Europe because the Allied leaders were weak and incompetent, if not downright dishonest and hypocritical, acting stupidly and too late. Had they stood firm against Hitler, had they assessed events more realistically, had they been honest and courageous, war might have been averted. In a sense, therefore, although Mosley does not say so, it was the Allied leaders rather than Hitler who were responsible for the war.

An altogether different approach to the outbreak of war is offered by Paul W. Blackstock in his study of prewar Soviet intelligence activities, *The Secret Road to World War II*.* The title promises more than Professor Blackstock can deliver; he never really explains just how Russia's intelligence operations led to war, and it is doubtful that they did. Actually, the contents of the book are more accurately described in the subtitle, *Soviet Versus Western Intelligence, 1921-1939*. Viewed from this more limited perspective, the volume is both informative and interesting, providing a detailed account of the struggle between Soviet and Western intelligence services, a fascinating story that will appeal to readers of James Bond. But Blackstock also attempts to establish a causal relationship between this struggle and Russian internal and foreign policies, citing approvingly the words of the late Soviet intelligence expert Boris Nicolaevsky that "in order to understand the real motives behind foreign policy one must study the battle of the secret intelligence services."

That intelligence played so large a role in international relations before the war is not at all clear, but the role of organized espionage in the affairs of

Europe during this period is important enough in itself to warrant study. In this respect, Blackstock's volume is more satisfying. He deals first with the organization known as the Trust, the apparatus that penetrated the anti-Bolshevist *émigré* groups abroad before 1927 and fed Western intelligence false information about the Soviet Union. In 1927 the activities of *émigré* groups operating inside Russia, says Blackstock, provided Stalin with the opportunity to manufacture a war scare and consolidate his control. From that time until 1937 the NKVD extended its network throughout Europe and engaged in terrorist acts, including the kidnapping of two *émigré* generals. In the final and most important part of his volume, Blackstock deals with the Tukhachevsky affair, prelude to the bloody purge of the Soviet high command, which he contends was based in part on information fed to Soviet intelligence from German sources. In Blackstock's view, the purge was a major factor in Stalin's plans for a working partnership with Nazi Germany and in Hitler's decision to attack Russia in June 1941.

By their nature, intelligence agencies do not advertise their activities and write no official histories, at least for publication. But intelligence operations make for fascinating reading, and there is a large literature on the subject, much of it written by former agents describing their exploits. Lyman Kirkpatrick's *Captains Without Eyes* belongs to this genre.* Formerly executive director of CIA and now a professor of political science at Brown University, Mr. Kirkpatrick seeks to demonstrate how failures in intelligence critically affected the outcome of five key episodes of the war. Despite the title and the jacket blurb describing it as a "controversial and unprecedented book" based on "previously unpublished records and personal experience," the volume is a rather routine study, based almost entirely on a limited selection of secondary sources and published documents, of five alleged intelligence failures. The emphasis in each of the case studies (*Barbarossa*, Pearl Harbor, Dieppe, Arnhem, and the Battle of the Bulge) is on intelligence, and even in this special area Mr. Kirkpatrick has little new to offer.

That intelligence plays a role in war should come as no surprise, but it is surprising to learn that *Barbarossa* (the German attack on Russia) and the Japanese attack on Pearl Harbor represent failures in intelligence. What went wrong at Pearl Harbor can hardly be attributed to lack of information. As Roberta Wohlstetter has demonstrated in *Pearl Harbor, Warning and Decision* (Stanford, 1962), it was not the lack of information that was responsible for the disaster, but rather the weakness of the entire machinery for evaluating and disseminating intelligence available from a variety of sources. Nor can one attribute the initial Nazi successes in Russia to the failure of intelligence. Stalin was informed of Hitler's intent in time to take preparatory measures to meet the attack, but for a variety of reasons he refused to do so.

By mid-1941 many of the Allied leaders Mosley indicts had faded from the scene, and leadership of the Western Allies had passed to Churchill and Roosevelt. The first meeting of these two and their chief military and civilian advisers off

the coast of Newfoundland in August 1941, four months before the United States entered the war, is the subject of Theodore Wilson's *The First Summit*, a title more reminiscent of the cold war than of US-British relations before the war.* Wilson's study is a carefully researched and detailed account of the background and preparations for the meeting (almost one-third of the volume) and of the wide-ranging political and military discussions that paved the way for the wartime alliance. In effect, Roosevelt promised Churchill all aid short of war and received in return Churchill's support for a general statement of war aims that became known as the Atlantic Charter. In practical terms the agreements reached at Argentia proved of limited importance and were soon modified by the exigencies of war. The real significance of the conference lay in the fact that it brought together for the first time the principal political and military leaders of the Western alliance and set the stage for the great wartime conferences that followed.

When Churchill sailed to Newfoundland to meet President Roosevelt in August 1941, the war in Europe was already two years old. Most of the Continent was under Hitler's control, and German forces were penetrating deep into Russian territory. Britain stood alone, and the outlook for the future was grim. The Mediterranean life line was threatened by Rommel's Afrika Korps and by the prospects of an assault on Gibraltar. If Russia were conquered, as many believed it would be, if Rommel were successful, and if Franco joined the Axis, Hitler would stand as the master of Europe and the Mediterranean world.

The story of the first two years of the war is not well known to Americans, for whom World War II begins in December 1941, just as for the Russians it begins in June of that year. Thus, much of the best writing on the early period of the war has come from European or British writers.⁴ The most recent of these is Henri Michel's *La Seconde Guerre Mondiale*, in the *Peuples et Civilisations* series edited by Professors Halphen and Sagnac.* Michel's work meets in all respects the high standards we have come to expect from that distinguished series. Projected in two volumes,⁵ the present work, *Les Succès de l'Axe*, covers the period of Axis success from the invasion of Poland to the German defeat at Stalingrad at the end of 1942. It is a masterly synthesis of operations in all parts of the world and of the plans and forces of the nations involved.

Les Succès de l'Axe is in no sense an operational history of battles and campaigns, but Michel understands fully the strategic and tactical aspects of warfare and writes about them with authority. M. Michel is a historian, not a soldier, and his view of war is that of the statesman. He never loses sight of the relationship between political aims and the use of military force to achieve these aims. His

⁴ The British official *History of the Second World War*, ed. Sir James R. M. Butler (London, 1952-) contains some of the best work in English for this period on both the strategic and operational levels in its separate subseries.

⁵ The second volume, *La Victoire des Alliés 1943-1945* (Paris, 1969), has already been published but is not included in this survey.

treatment of military operations is thus subordinated to their strategic and political purposes, and it is these purposes rather than the operations themselves with which he is largely concerned. He is aware also of the importance of domestic issues, economic factors, manpower problems, and weapons in the conduct of war and treats each in its proper place. He writes authoritatively of German occupation policy, of the concentration camps, and of the mass murder of Jews and others. In short, this is military history in the best sense, combining the political, economic, social, and military aspects of war into a single tapestry, woven with grace and skill and a high order of scholarship.

The most decisive event in these early years of World War II was the German defeat of France, one of the most brilliant military victories in history and the most decisive defeat in French history. Though the French have never published an official history and records on the French side are scanty, there have been a large number of excellent accounts of this disaster, including such outstanding works as Marc Bloch's *Strange Defeat* (London, 1949) written soon after the event, Jacques Benoist-Méchin's *Sixty Days That Shook the West* (New York, 1963), Major General Sir Edward L. Spears's *Assignment to Catastrophe* (London, 1954), and General André Beaufre's *The Fall of France* (Paris, 1965). All sought an explanation for the dissolution of the French army, before 1940 considered the best in Europe, and found it variously in the treachery of the fifth column, a mistaken military policy, an erring strategy, the poor morale of the French soldiers, and the weakness of their leaders. Still the search for the causes of defeat continues. In the past year there have been at least three new studies in English: William L. Shirer's massive tome, *The Collapse of the Third Republic* (New York, 1969), Guy Chapman's largely operational *Why France Fell* (New York, 1969), and Alistair Horne's *To Lose A Battle, France 1940*,* third in a trilogy that includes perhaps the best account in English of the siege of Paris in 1870 and one of the finest works on World War I.⁶

To Lose A Battle is not only a fitting climax to Horne's trilogy of Franco-German rivalry, each volume of which is focused on a single decisive battle, but also the most satisfactory of the recent works on the defeat of France, thoroughly and carefully researched, beautifully written, and well mapped and illustrated. Horne begins his story with the victory parade in Paris on July 14, 1919, and then skillfully traces political and social life in France during the interwar years, the stultifying effect of the reliance on the doctrine of defense exemplified in the Maginot line, and the failure of leadership of the aging heroes of World War I—Gamelin, Georges, Weygand, and Petain. Their failure is sharpened by contrast with such youthful and vigorous Germans as von Manstein and Guderian who, following the lead of Liddell Hart, fashioned the armored formations that were largely responsible for German victory.⁷

⁶ Horne, *The Fall of Paris, The Siege and the Commune: 1870-71* (New York, 1965), and *The Price of Glory, Verdun 1916* (New York, 1963).

⁷ See B. H. Liddell Hart, *The Tanks, The History of the Royal Tank Regiment, 1914-1945*

The battle for France, says Horne, was decided between May 10 and May 20. The story of these ten days forms the heart of Horne's volume. It begins with the coordinated air and ground attack by the Luftwaffe and Wehrmacht and the advance across the Belgium and Luxembourg borders and through the Ardennes. On the first day the Germans captured the great Belgian fortress of Eben Emael, guarding Liege and the Allied position along the Albert Canal. The Germans had planned well, and everything worked to perfection. At one point Guderian issued to his troops orders used earlier for a war game, changing only the time. By May 14 the Germans had crossed the Meuse; by the sixteenth they had broken through the defending forces, outflanked the Maginot line, and were pushing the Allied armies toward the coast. Wrote Rommel jubilantly in his diary, "The way to the west was now open. . . . It was not just a beautiful dream. It was reality."

On May 20 the first Panzer units reached the sea, initiating an encirclement that virtually ended the battle. But at the last moment, for reasons not yet fully understood, Hitler halted his troops, permitting over 300,000 British and French soldiers to escape across the Dunkirk beaches. But neither this grave error nor the bitter battles that followed could have any effect on the final outcome. On June 21, a week after German troops had taken Paris and pressed southward as far as Nantes and Lyons, the surrender was signed at Compiègne.

France conquered, Hitler sought to bring Britain to her knees;⁸ when that effort failed, he turned his attention eastward to begin a campaign that was ultimately to prove his undoing. Years afterward, faced with defeat, he is reported to have said that his great mistake was that he did not turn south and attack Gibraltar immediately after the defeat of France, "taking advantage of the enthusiasm we had aroused in Spain and the shock to which we had subjected Britain." This thought provides the theme of Charles Burdick's *Germany's Military Strategy and Spain in World War II*.^{*} The opportunity that existed in the summer of 1940 was never repeated, and all Hitler's plans for binding Spain to the Axis and gaining control of the Western Mediterranean came to naught.

Burdick's volume is a study of strategy that was never implemented and plans that were never used: *Felix* and *Isabella* in 1940-41 for a full scale assault on Gibraltar with Spanish cooperation; *Ilona* and *Gisela* in 1941-42 for a holding action in the event of an Allied landing on the Continent; and *Nürnberg* in 1943 for a last-ditch defense in the Pyrenees. This succession of plans is, in a way, a measure of the deterioration of Germany's military fortunes and the increasingly high price Franco demanded for cooperation. In describing these plans and the

(London, 1959); Heinz Guderian, *Panzer Leader* (New York, 1952); F. W. von Mellenthin, *Panzer Battles* (Norman, 1956); and Eric von Manstein, *Lost Victories* (Chicago, 1958).

^{*} This story is told in a number of popular works on Dunkirk and the Battle of Britain. The British official histories (Basil Collier, *The Defense of the United Kingdom* [London, 1959], and L. F. Ellis, *The Battle for Normandy* [London, 1962]) are still the most complete accounts.

strategy that lay behind them, Professor Burdick has provided a revealing picture of the German high command and its relations to Hitler as well as a detailed study of German military planning. Relying almost entirely on primary sources and interviews with a number of senior German officers, Burdick has produced a work of impressive scholarship. But no matter how well done, a description of plans that were never used can constitute no more than a footnote to the history of the war in Europe.

Barbarossa, the plan that Hitler did follow after the Battle of Britain, was an even greater mistake than was his failure to follow *Felix*, for it led ultimately to the one thing that he had hoped to avoid, a war on two fronts. *Barbarossa* began with the invasion of Russia in June 1941 in a drive that took German forces to the gates of Moscow and deep into the Crimea by the end of the year. Fought quite separately from the war in the West, the Soviet-German war was by all odds the biggest, bloodiest, most decisive theater of World War II, dwarfing the Allied effort in the West and involving vast armies along a front stretching over a thousand miles.

There is comparatively little available in English on the war in the East. Two recent works covering the entire period of the war, both sound and eminently readable, are Alan Clark's *Barbarossa* (New York, 1965) and Alexander Werth's *Russia at War* (New York, 1964).⁹ Among more specialized accounts are those dealing with such key battles as Leningrad and Stalingrad.¹⁰ The number is not large, but it includes one of the best volumes yet written on the war in Russia, Harrison Salisbury's magnificent account of the siege of Leningrad, *The 900 Days*.*

No incident of World War II, with the possible exception of the gas chambers of Auschwitz and Belsen, can compare in human suffering to the epic defense of Leningrad. From October 1941 to January 1944 the city lay under the Nazi guns, virtually shut off from all contact with the outside world. The worst time was the winter of 1941-42, one of the coldest on record. Leningrad, already battered by shells, was without heat or light, and what food reserves there were quickly disappeared. That winter the daily death toll reached five thousand, and before the siege was lifted almost half the three million inhabitants of the city perished from starvation, cold, disease, or gunfire. Cannibalism was not uncommon, and human flesh found its way into the market place. At one point, Stalin, who hated the city, thought of abandoning it, destroying its industry, bridges, and everything the enemy might use. But his orders were never carried out, and the survivors held on grimly for another two years. No city, not even Warsaw and Hiroshima, suffered as did Leningrad during those two years. And

⁹ The Soviet official *History of the Great Fatherland War of the Soviet Union 1941-1945* (Moscow, 1960-63) has been translated into German but is not available in English.

¹⁰ I. V. Chuikov, *The Battle for Stalingrad* (New York, 1964); Leon Goure, *The Siege of Leningrad* (Stanford, 1962).

in the end, three years after the war, thousands of the survivors fell victim to Stalin's hatred in the purge known as "the Leningrad affair."

Mr. Salisbury, former Russian correspondent and now assistant managing editor of the New York *Times*, spent a good many years collecting the material for this book and talking to the survivors. (The bibliography of Russian sources alone covers thirteen closely packed pages.) His love for the city and its people is apparent, but he is not blinded by his affection. He includes the sordid and the ugly as well as the beautiful and the heroic. Nothing has been left out. The pace is slow and magisterial—almost half the book is devoted to events preceding the actual siege, including one of the best analyses of Russia's position at the start of the war. The volume is studded with detail and statistics, yet the narrative never palls. *The 900 Days* may not be the final word on the siege, but it is not likely soon to be surpassed. Certainly it is the finest testament yet written to the heroism of the people of Leningrad.

One of the early commanders of the Leningrad defense and the outstanding Russian general of World War II was Marshal Zhukov. His memoirs, recently translated into German and extracted for an American audience, are a significant addition to the memoir literature of the war, especially as so few Russian generals have dared to publish memoirs. The German edition of the memoirs is much more extensive than the American version published under the title *Marshal Zhukov's Greatest Battles*.* In his account, almost half of which is devoted to the years before World War II, Zhukov writes about his early life as a member of a poor peasant family in a village not far from Moscow and of his service in the First World War as a noncommissioned officer in the Novgorod Dragoons, for which he was twice cited for bravery. After joining the Red Army in October 1918, he fought in the Civil Wars under Marshal Timoshenko, rising to the rank of squadron commander in the cavalry.

During the interwar years, young Zhukov learned the trade of armored warfare, attended a succession of military schools in Russia and Germany, went to Spain as an observer in 1936, and then to China. In 1939, as commander of the First Army Group, he won a decisive victory over the Japanese in the battle of Khalkhin-Gol, earning a reputation as one of the most capable young officers in the Red Army. Next came appointment as deputy to Timoshenko, commander of the Ukrainian Military District, and then service in the war with Finland. In the shuffle of commands that followed the Winter War, Zhukov was given his old chief's command in the Ukraine, where the party chief was Nikita Khrushchev. Now a senior commander and recognized expert on tank warfare, Zhukov could speak out more boldly about the weaknesses of the army. In January 1941 he was summoned to Moscow by Timoshenko and made chief of staff of the high command. From this position he sought to strengthen the Red Army for war, but Stalin, convinced the Germans would not attack, prohibited any move that might alarm Hitler. And when the Germans finally did attack, it was Timo-

shenko and Zhukov rather than Stalin who directed Russia's military forces for the first few weeks of the war.

Zhukov's name has become associated with many of the most famous Russian battles of World War II. As Harrison Salisbury observes in his splendid introduction to *Marshal Zhukov's Greatest Battles*, Zhukov emerged "as the master of disaster . . . whose terrible temper, iron will and savage determination . . . somehow succeeded in bringing the German war machine to a grinding halt and in setting the stage for a counterblow." His first victory came in September 1941 at Yelna, 220 miles west of Moscow. But his greatest successes, as he describes them, were the battle of Moscow in 1941-42; the battle of Stalingrad, which marked the end of the German advance; the battle of Kursk in the summer of 1943, which saw the strategic initiative pass to the Russians; and the battle of Berlin, final battle of World War II. Each of these battles was a watershed in the titanic struggle between Russia and Germany, involving millions of men, huge casualties, and thousands of tanks and artillery pieces. In conception and scope they exceeded any of the battles on the Western front and established Marshal Zhukov as perhaps the greatest of the World War II generals.

In the West, Nazi troops remained unchallenged, extending their control to Italy, the Balkans, and North Africa. But with the entry of the United States into the war in December 1941, Allied prospects for the future brightened considerably. Even while the Japanese were advancing in the Pacific, the British and American planners began to plot their future course.¹¹ Despite the vast amount of published material on this period of the war, there are few general accounts of the Allied drive from North Africa to the surrender of Germany that successfully weave ground, air, and naval operations with the strategic and logistic aspects of the war into a coherent narrative. Two recent efforts to do so offer an interesting comparison. The first is *Krieg Im Westen* by Olaf Groehler,* an East German who takes a Marxist-Leninist line; the other is Charles B. MacDonald's *The Mighty Endeavor*,* a well-written account of the war, focused largely on American forces, by the deputy chief historian of the Department of the Army.

Olaf Groehler's work is probably of greater interest to Kremlinologists than to serious historians. It consists of two loosely connected parts—the first, a communist critique of Western literature on the Allied planning for a second front; the second, a description of German defenses of the French coast and Allied preparations for the invasion of the Continent. The author is familiar with all the important Western works on the war (at least, he cites them) and has

¹¹ The literature on the strategy of the war is extensive. The best accounts are the relevant volumes in the British and US Army official histories. Excellent brief accounts are K. R. Greenfield, *American Strategy in World War: A Reconsideration* (Baltimore, 1963); Hanson W. Baldwin, *Great Mistakes of the War* (New York, 1950); and S. E. Morison, *Strategy and Compromise* (Boston, 1958).

used in addition the German military archives in Potsdam and the German military records captured by US forces and microfilmed by the AHA.¹²

The first part of the volume depicts the US-British debate over strategy as a struggle between the two capitalist powers for commercial supremacy and control of the sources of raw material. American strategy is explained in terms of the interests of financial groups within the United States. The Soviet Union is uniformly portrayed as the true leader of the democratic world, fighting heroically and virtually alone in the cause of freedom. The landing at Normandy is described as a relatively bloodless affair, made possible by the attrition of the German armies in the East and the sacrifices of the Red Army. The most useful and interesting part of the book is that dealing with German preparations for and reactions to the invasion of France. This portion of the volume is based largely on archival material available in East Germany and contains detailed information on such matters as the disposition of German defenses, the movement of units, and losses. The invasion itself and the subsequent advance of Allied forces across France and into Germany are described briefly in a concluding chapter.

The chief virtue of Charles MacDonald's *Mighty Endeavor*—the title comes from Roosevelt's message on D-Day, June 6, 1944—is that it presents in a single volume an accurate, swift-paced, and readable account of American participation in the war against Germany. There are no revelations, no new interpretations or insights in this book. But nothing of significance has been omitted, and that is no mean achievement. Opening with a chapter on prewar strategic planning, MacDonald describes the development of the American high command, a subject too often ignored; the war against the U-boats to secure the life line to Britain; and the confused planning during the early months of the war that culminated in the landings in North Africa. In eight chapters he covers the war in the Mediterranean and in another fifteen the war in Europe.

The problems of organization, level of treatment, and subject matter in a volume of this scope are formidable, and Mr. MacDonald has generally solved them in a satisfactory manner. He has avoided the major pitfalls and has allocated space proportionately and justly, although one might disagree with his emphasis on ground operations as compared to the air and naval war. He moves easily from the lofty levels of the Combined Chiefs and theater headquarters to the action of small units on the battlefield, from combat to command, from strategy to the mundane problems of supply. Deftly and with a sure touch he covers the essential elements in his story, avoiding superficiality and tedious detail. Occasionally the writing is overdramatic, the issues too sharply drawn. But the judgments are generally sound and the research thorough. In almost every respect, *Mighty Endeavor* is a thoroughly satisfactory general account of the war the Americans waged in Europe.

¹² For a description of these records, see the National Archives and Records Service, *Guides to German Records Microfilmed at Alexandria, Va.* (Washington, 1958-), and D. H. Perman, "Micro-filming of German Records in the National Archives," *American Archivist*, XXII (1959), 433-43.

Operations in the Mediterranean theater began with the unsuccessful Italian effort to drive the British out of Africa, expanded when German forces under Rommel came to the rescue of the Italians, and then spread rapidly after the United States became a belligerent. Thus, the Mediterranean was the scene of active operations almost from the start of the war down to the surrender. Moreover, it was the subject of the most prolonged and publicized debate over strategy between the British and the Americans.

The central issue in the postwar controversy over Mediterranean strategy is the charge, most persuasively argued by Chester Wilmot and supported by Lord Alanbrooke's memoirs,¹³ that the British pursued from the start a deliberate and coherent strategy that had as its aim a major move into the Balkans as an alternative to the American strategy of a cross-channel assault. So widely held is this view and so firmly embedded in American official and unofficial histories of the war that it is virtually accepted as a self-evident truth. It is not surprising, therefore, that Trumbull Higgins in his recent study of the Anglo-American controversy over Mediterranean strategy, appropriately entitled *Soft Underbelly*, adopts the customary American position concerning British strategy.* As a matter of fact, this position is particularly appropriate to the anti-Churchillian thesis of the volume. Mr. Higgins' real target is Winston Churchill, and by identifying the prime minister as the chief architect of a mistaken British Mediterranean strategy and the strongest advocate of an ill-advised Balkan invasion, Higgins is able to continue the attack that he began some years ago with his volumes on Churchill's role in the Dardanelles campaign of World War I and the second front in World War II.¹⁴ It should be said for Mr. Higgins, however, that his criticism of British strategy is carefully constructed and well researched, although, as in his earlier works, he has relied largely on secondary materials, especially the official histories. Nor is his attitude toward Winston Churchill carping or hostile. On the whole, *Soft Underbelly* is the best of the three volumes Higgins has written on World War II.

The shortest and one of the best studies of this controversial subject is Michael Howard's *The Mediterranean Strategy in the Second World War*.* Originally delivered as the Lees-Knowles lectures at Cambridge in 1966, this seventy-page volume by one of England's leading military historians presents as lucid and dispassionate an explanation of Allied strategy as can be found in many larger works. There is no question where Professor Howard stands in this controversy. He denies flatly that there was any such thing as a coherent British strategy for the Mediterranean or a grand design for the defeat of Germany by way of the Balkans. He further asserts that the British never had any scheme to replace the

¹³ Chester Wilmot, *The Struggle for Europe* (New York, 1952); Arthur Bryant, *The Turn of the Tide, 1939-1943* (London, 1957), and *Triumph in the West* (London, 1959), both based on the diaries of Field Marshal Lord Alanbrooke, chief of the imperial general staff.

¹⁴ Trumbull Higgins, *Winston Churchill and the Dardanelles* (New York, 1963), and *Winston Churchill and the Second Front, 1940-1943* (New York, 1957).

cross-channel attack with a Balkan invasion. With precision and clarity he traces the development of strategy in the Mediterranean from the early period of the war through the campaign in Italy.

British operations in the Mediterranean, Professor Howard argues, grew out of the necessity for protecting the Middle East supply of oil, expanded in 1942 as a result of Roosevelt's desire to get American forces in action against the Germans as soon as possible and continued thereafter in order to utilize the forces available until sufficient resources could be assembled for the invasion of north-west Europe. But by 1943, as Mr. Howard recognizes, the British chiefs had begun to view operations in the Mediterranean as an end in themselves. It was only because of the single-minded insistence of the Americans that the British finally agreed to set a definite date for the invasion of the Continent.

Professor Howard is equally critical of British and Americans, of Churchill and Roosevelt, Alexander and Mark Clark. He doubts that the invasion of southern France, which the Americans insisted upon, contributed materially to the defeat of Germany. He thinks it is equally doubtful that the troops used for that landing could have been utilized more effectively in Italy, as the British wanted. The idea of an assault through the Ljubljana Gap in Yugoslavia toward Vienna—the soft underbelly—was never seriously proposed as a strategic alternative to *Overlord*, Howard asserts. And even had such an assault been attempted, he doubts that it could have prevented the postwar extension of Soviet influence in East Europe or provided a quicker and less expensive way of winning the war.

Operations in the Mediterranean area have received almost as much attention as has strategy there. The Mediterranean was the burial ground for more than one reputation and the proving ground for many of the major figures of the war—Eisenhower, Montgomery, Alexander, Clark, Patton, and Rommel, all of whom have published their memoirs or been the subject of biographies. Of these, Rommel is perhaps the most interesting if not the greatest. Certainly he belongs in the company of the great cavalry captains, a master of armored warfare, as Ronald Lewin makes clear in his carefully researched and well-written study, *Rommel As Military Commander*.* A professional soldier whose career spanned the two world wars, Rommel's greatest achievements came in North Africa, where even in defeat he displayed outstanding qualities of generalship. Called back to Europe, he became convinced that Hitler's policies were causing Germany to lose the war. Just what role he played in the plot against the Führer is not clear, but when Hitler's emissaries offered him poison, he accepted his fate and committed suicide.

Lewin's volume has many merits. Though his study is limited to Rommel's military career, he tells us much about Rommel the man. Lewin writes well and succeeds admirably in creating a sense of the battlefield as well as explaining the larger problems of war that the general faced. Finally, the volume is handsomely printed, with excellent maps and illustrations to support the text.

The campaign in Italy was one of the hardest fought of the war, but it is not at all clear that it was worth the cost in men and material. Like the strategy that brought Allied troops into Italy, the conduct of the campaign has also become a subject of controversy. To the large literature on the subject we now add three more volumes, one covering the first eight months of the war in Italy and two the battle for Rome. The first, Martin Blumenson's *Salerno to Cassino*, is a volume in the army's official history of the war and, like the other volumes in that outstanding series, is based almost entirely on original sources, contains excellent maps and sketches, and is lavishly illustrated.* Blumenson, who writes with a flair not usually found in official histories, carefully sets the stage for the Italian campaign in five fast-moving chapters, then traces the progress of the campaign from the landings at Salerno in September 1943 through the hard-fought battles along the Winter Line to the battles for Anzio and Cassino. The volume ends as the Allies prepare to launch a new offensive in the spring of 1944 to break out of the Anzio beachhead and through the Gustav line in the drive toward Rome. Where appropriate, Blumenson weaves into the tactical narrative sections on strategy, air and naval operations, and German plans and dispositions.

Where Blumenson leaves off, W. G. F. Jackson's *The Battle for Rome* picks up.* After a rather lengthy warm-up on the period from February to May 1944, he describes in some detail how the Allies worked their way forward slowly through the difficult terrain against the strong, well-led forces of General Kesselring to the outskirts of Rome. On June 4, just two days before the invasion of Normandy, Allied forces entered the city. General Jackson is a British officer who fought in Italy, and his volume is a frank effort to rescue operation *Diadem* (code name for the capture of Rome) from the shadow of *Overlord* and to accord General Alexander the credit he surely deserves for preventing the campaign in Italy from becoming a stalemated war of attrition, like World War I. Though he is fair enough to Mark Clark, Jackson is clearly in the British corner.

In the American corner defending General Clark and emphasizing the American contribution are Robert H. Adleman and Colonel George Walton, authors of *Rome Fell Today*.* Their volume makes no pretense to being a scholarly or definitive study. It is, rather, an entertaining, highly spiced narrative about the well-known controversies of the Italian campaign—the Rapido River crossing, Anzio, and Mount Cassino—written generally from the level and point of view of Clark's Fifth Army headquarters. It is chatty, superficial, and often wanders from the subject, and while it answers none of the thorny questions about the Italian campaign, it provides some interesting stories and sidelights on the war. And it may well be that the outstanding general in Italy was neither Alexander nor Clark, but Kesselring.

The high point of the war from the Allied point of view was the invasion of Western Europe and the subsequent drive across France and the Lowlands. Two years of planning and preparation preceded the landing, and this aspect of

the campaign has received almost as much attention as the landing itself.¹⁵ An essential element in the preparations and, some believe, a prerequisite to the success of the invasion was the combined bomber offensive against Germany, designed to break the will of the German people and disrupt German production and transportation. This subject has been fully covered in the official histories: the four-volume British series, *The Strategic Air Offensive Against Germany* (London, 1961) by Sir Charles K. Webster and Noble Frankland, and the second and third volumes of *The U. S. Army Air Forces in World War II*, edited by W. F. Craven and J. L. Cate (Chicago, 1949, 1951). Anthony Verrier's *The Bomber Offensive* covers the same ground, but in less magisterial fashion and in briefer span.*

According to the theorists of air power, the bombardment of Germany from 1939 to 1945 should have produced decisive results if not the defeat of the enemy. Actually, it produced neither, although it certainly made a contribution to victory. Why did it fail? Was it worth the heavy price paid? These are the questions Mr. Verrier tries to answer. The central problem, he believes, was the lack of strategic control at the highest level, a lack that made it possible for both the British and American air commands to engage in the area bombing of German cities rather than the destruction of carefully selected strategic targets. In pursuing this strategy of devastation, the Allied air forces developed long-range fighter escorts for daylight bombing and perfected the tactics of destruction but failed to achieve their purpose. Just what they did accomplish—other than to crush the German air force, a task that need not have required the resources and the men used to lay Germany waste—is still not clear. There is a lesson here for our own time, for Americans still seem intoxicated with the idea that air power by itself can achieve decisive results.

Another essential element in the preparation for the invasion of the Continent was the security of the Atlantic life line on which the flow of men and material and the very existence of Britain depended. The battle of the Atlantic is essentially a naval story, superbly described on the American side by S. E. Morison in his *History of U. S. Naval Operations* and on the British side by S. W. Roskill in *The War At Sea, 1939–1945*.¹⁶ Herbert Werner's *Iron Coffins* is only a footnote to these two definitive works, but it is an exciting and useful account nevertheless.* Mr. Werner, now an American citizen, is a former U-boat commander, one of the few who survived the war, and this is his personal account of his wartime experience.

¹⁵ See Dwight D. Eisenhower, *Report by the Supreme Commander to the Combined Chiefs of Staff, 6 June 1944–May 1945* (Washington, 1946); General Sir Frederick E. Morgan, *Overture to Overlord* (London, 1950); and several of the volumes in the official U. S. Army in World War II: the two volumes of R. M. Leighton and R. W. Coakley, *Global Logistics and Strategy, 1940–1943* (Washington, 1955), and *Global Logistics and Strategy, 1943–1945* (Washington, 1968); Gordon Harrison, *Cross-Channel Attack* (Washington, 1951); and Roland Ruppenthal, *Logistical Support of the Armies* (Washington, 1953–59).

¹⁶ The relevant volumes in the Morison series are *The Battle of the Atlantic* (Boston, 1947), and *The Atlantic Battle Won* (Boston, 1956); in the British series, see Vols. II and III, pt. 1. An excellent general account of the war against the submarine is Nicholas Monsarrat, *The Cruel Sea* (New York, 1951).

In addition to the realistic picture of life aboard a U-boat and the way in which these underwater craft operated, the volume shows how those German officers who spent most of their wartime years at sea refused at first to accept the truth about their leaders and then became disillusioned when they could no longer evade the truth.

Although there are a number of studies on special aspects or specific operations of the war in the West, there are only a few satisfactory accounts of the war as a whole, from Normandy to V-E Day. The US Army's official history devotes at least a half-dozen volumes to the American side alone; the Canadian side, told by Charles P. Stacey, is compressed into a single volume.¹⁷ The British official history devotes two volumes, entitled *Victory in the West*, by Major L. F. Ellis and others, to this part of the war. The first volume, *The Battle for Normandy* (London, 1962) describes the Allied landing on the Continent and operations in France down to August 1944. The second volume, *The Defeat of Germany*, picks up the story in September 1944, when hopes for an early victory were high, and carries it through the bitter setbacks in the Lowlands and the Ardennes in the fall and winter of 1944 to the final battle for Germany.*

Like the first volume, *The Defeat of Germany* deals largely with the operations of the British Twenty-first Army Group but includes enough of the operations of American troops and of air and naval forces to make the entire campaign in France and Germany understandable. Nor are the larger problems of the campaign overlooked. Among these were the problems of logistics, which had much to do with the inability of the Allied forces to exploit their victory during the autumn of 1944, and the differences between Eisenhower and Montgomery over strategy (the broad versus narrow front) and command. The maps and illustrations are excellent, and there are a number of useful appendixes listing the forces involved on both sides, casualties, civil affairs, and other matters. However, like the other volumes in the British official history, the volume contains neither notes nor bibliography.

Major General H. Essame's *The Battle for Germany* covers virtually the same ground as the official history and, like it, is focused largely on the operations of the Twenty-first Army Group.* But unlike Major Ellis, General Essame, who commanded an infantry brigade during the campaign, is more incisive in his judgments and more concerned with the strategic and political background of battle than with the battle itself, although he chronicles each action with a sure hand and a keen sense of what is tactically important. Essame's volume is better written than is Ellis' official history, and, though not as deeply researched or as thorough, it moves at a faster pace.

In writing about the differences between the British and Americans, Essame's sympathies are clearly on the side of General Montgomery, whom he admires

¹⁷ Charles P. Stacey, *The Victory Campaign: The Operations in Northwest Europe, 1944-1945* (Ottawa, 1960). An excellent summary is the semi-official history by John North, *Northwest Europe, 1944-1945* (London, 1953).

enormously. He believes that the American commanders generally, and Eisenhower in particular, were shortsighted in their conduct of the war, blind to the political implications of military operations, and closely guided by American public opinion. But he is also aware of the pressures under which Eisenhower labored and places a large share of the blame on the Chiefs of Staff and their political superiors for failing to give him proper guidance. On the other hand, Montgomery, he finds, was usually correct, even if he behaved like "an Old Testament prophet at loggerheads with the Kings of Israel." Still, whether one agrees with these judgments or not, this is a first-rate and highly readable account of the campaign that ended with the surrender of Germany.

Virtually every battle in the war against Germany, from the beaches in Normandy to the last battle before Berlin, has been described in detail, some a number of times by both official and unofficial historians and by many of the participants as well. After the invasion itself, the operation in which Americans have displayed the greatest interest is the German counter-offensive in the Ardennes in December 1944, usually called the Battle of the Bulge. The earliest book on this subject was Robert E. Merriam's *Dark December* (Chicago, 1947); the most recent is John S. D. Eisenhower's *The Bitter Woods*.^{*} Neither equals the fullness or detail of the official Army account by Hugh M. Cole, *The Ardennes* (Washington, 1965), or the excitement of John Toland's *Battle: The Story of the Bulge* (New York, 1959).

The idea for the counter-attack was Hitler's, and it came with startling suddenness to an army that had fought its way in six months to the borders of Germany and was preparing to mount a new offensive, one to the north under Montgomery and another to the south under Patton. Between the two was the difficult terrain of the Ardennes, thinly held by inexperienced American troops. Hitler's objective was to reach Antwerp, and it seemed for a time that he might make it and reverse the course of the war. Not only did he create a dangerous bulge in the Allied line centering around Bastogne, but he also brought on a serious crisis in the relations between the American and British commands. This was perhaps the most critical moment of the campaign, and it is a tribute to the strength of the coalition that it survived both crises and went on to win the war.

The difficulties of dealing with the Battle of the Bulge objectively and honestly, involving as it does serious problems of intelligence, Allied relations, command, and the operations of units ranging in size from armies to companies, are considerable. For the son of the supreme commander these difficulties would seem almost insurmountable, yet John Eisenhower has managed to solve them. He is objective and accurate in his description of the relations between his father and Montgomery, fair and moderate in his judgments (except possibly when he deals with the question of bypassing Berlin), and meticulous in his coverage of the battle on all echelons and by both sides. If anything, his description of the fighting in the Ardennes is too detailed and too long. John Eisenhower may not have written the

definitive or most readable account of those dark days of December 1944, but his work is certainly a major contribution to the subject.

An operation of an entirely different sort was the drop of the ten-thousand-man First Airborne Task Force into the Riviera region on August 15, 1944, to guard the flank of the landings in Southern France and to disrupt enemy communications behind the lines. The story of this operation is told in *The Champagne Campaign*,* so named because the paratroopers partook liberally of the champagne stocks in the region, by Robert Adleman and Colonel George Walton, authors of *Rome Fell Today* and *The Devil's Brigade* (Philadelphia, 1966). Like their earlier books, this volume is an informal, chatty mixture of personal narrative and tales of heroic exploits on the battlefield and in the bars of Nice. The authors place almost equal emphasis on both and seem as proud of the off-duty accomplishments of the paratroopers as they do of their fighting abilities. Judging from some of their activities, the French could not have found much difference between their German conquerors and their American liberators.

Except in the councils of the Combined Chiefs of Staff, where overall strategy was made and resources allocated, there was little connection between the war against Germany and the war against Japan. They were, in effect, two separate wars. And just as the war against Germany consisted of two separate and uncoordinated fronts, one in the East and one in the West, so did the conflict with Japan consist of virtually unrelated wars, one fought in China and Southeast Asia and the other in the Pacific. The first was an Allied war involving American, British, and Chinese forces in a complex organization in which the American commander served as deputy to one British superior and chief of staff to Chiang Kai-shek. The responsibility of US troops in the area was almost entirely logistic, and few US combat forces saw service on the Asiatic mainland. The war in the Pacific, on the other hand, was very largely an American effort in which the navy and marines played a major, if not the major role. Here, too, there was a division of effort, with MacArthur heading a predominantly army theater of operations in the Southwest Pacific and Admiral Nimitz a predominantly naval theater stretching from Hawaii to the China coast and from New Zealand to Alaska.

To encompass within a single volume the entire panorama of the war in Asia and the Pacific presents difficult problems.¹⁸ One of the more successful efforts to do so is Basil Collier's *The War in the Far East, 1941-1945*.* Author of a number of works on World War II, including a volume in the British official history and a one-volume survey of the entire war,¹⁹ Collier is an experienced military historian with a wide knowledge of the war. He is, moreover, a skilled writer with a rare talent for brevity and clarity and the ability to explain complex matters simply

¹⁸ A recent summary account is that of the Australian journalist Charles Bateson, *The War With Japan, A Concise Summary* (East Lansing, 1968).

¹⁹ Collier, *Defense of the United Kingdom* (London, 1957), and *The Second World War* (New York, 1967).

and clearly. (For example, his ninety-four-page account of events in the Far East from 1916 to 1941 is a model of succinctness and lucidity.) It is these virtues rather than original research or fresh interpretations that give this volume its particular merit.

As a Britisher writing about a war that Americans regard as peculiarly their own, Mr. Collier places somewhat more emphasis on operations in Southeast Asia than one usually finds in works by American historians. In describing the Japanese offensive during the first six months of the war, he allots four chapters to British operations and only one brief chapter to Pearl Harbor, the Philippines, Wake, and Guam. Whether or not one agrees with this emphasis, it is at least a useful antidote to the usual American-oriented treatment of the period. Moreover, Collier is more at home with British sources than he is with the much larger body of American materials on the war, much of which he has failed to use. The student of the war against Japan will learn little from this volume, except perhaps the merits of brevity and elegance of style, but for those seeking a readable and accurate summary of the war in the Far East, this volume fills the purpose admirably.

Strangely enough, there is no single volume on the Pacific war covering the operations of the army, navy, and air forces of all the nations involved. Morison devotes nine of the volumes in his history of the US Navy in World War II to the Pacific;²⁰ the army history, a total of eleven (with three additional volumes on the China-Burma-India theater); and the marines, a projected five volumes, the fourth of which, *Victory and Occupation*, by Benis M. Frank and Henry I. Shaw, Jr., has recently been published.* This thick volume deals with marine operations on Okinawa, in North China at the end of the war, and in the occupation of Japan. In addition to numerous excellent maps, sketches, and illustrations, the volume includes a large number of appendixes. There is little new in the detailed account of the Okinawa campaign, the subject of an earlier full study, but the chapters on North China, the concluding chapter entitled "A Final Accounting," and a sixty-page appendix on the experiences of marine prisoners of war provide fresh and significant material on the Pacific war.

The most significant naval developments of World War II were the emergence of the fast carrier task force as the main striking element of the fleet, replacing the battleship, and the efforts of the naval aviators to secure a command status that would ensure their control and proper employment of this new weapon. The development of the carrier and of carrier doctrine, of its employment in World War II, and of the struggle between the air and battleship admirals for control of the fleet is described in detail by Clark G. Reynolds in *The Fast Carriers*.* Professor Reynolds leaves no doubt about where he stands in the fight between the air and battleship admirals, and he pulls no punches in criticizing Spruance, Halsey, and others for their handling of the carriers and S. E. Morison for his "superficial

²⁰ Samuel Eliot Morison, *History of United States Naval Operations in World War II* (Boston, 1947-62).

treatment of naval aviation" in his naval history of the war. Mr. Reynolds' treatment of the strategy of the war against Japan is that of the aviators, and like them he finds the division of the Pacific into two theaters wasteful of resources and time consuming. The contention that Japan could have been defeated more quickly by a blockade and bombardment strategy than by the invasion strategy finally adopted is a familiar one, but unfortunately it cannot be proved. At any rate, neither strategy was used; the atomic bomb made both unnecessary. Despite its evident bias, Reynolds' volume has real value as a definitive account, based on primary sources, of the development of carrier doctrine, organization, and tactics and of the bitter struggle between the well-entrenched battleship admirals and those who led the fast carrier forces.

If for Americans the first six months of the war bring back memories of Pearl Harbor, Wake Island, Bataan, and Corregidor, for the British they evoke bitter thoughts of the *Prince of Wales* and the *Repulse*, retreat in Malaya and Burma, and the final humiliation of Singapore. There is a large literature on Singapore, one of the worst disasters in British history and the subject of considerable controversy, and on the campaigns in Malaya and Burma. Much of this writing is British, and the early volumes in the British official history cover these operations as well as the subsequent campaign in Burma, scene of General Stillwell's memorable retreat in 1942. One of the best accounts of the campaign in Burma is General Slim's *Defeat Into Victory* (London, 1956). Now one of Slim's division commanders, Geoffrey Evans, has written a brief and admiring biography of his former chief that adds little to what the general has already written in his own memoir and is not nearly its equal in literary style.*

General William Slim, later to become a field marshal and governor general of Australia, spent most of World War II in Burma, rising from command of a corps under General Alexander in 1942 to command of the British Fourteenth Army, a multinational force consisting of Gurkhas, Indians, Africans, and Chinese as well as British troops. It was this army that Slim reorganized after its defeat, imbued with fresh spirit, retrained and equipped, and finally led in a thousand-mile advance through the most difficult kind of terrain and a series of hard-fought battles across the mile-wide Irrawaddy River to capture Mandalay and Rangoon and regain Burma for the Allies. The campaign has been described as one of the most brilliant of the war, and it was Mountbatten's judgment that Slim "was the finest general World War II produced." His biographer agrees wholeheartedly with this judgment, and his book is a convincing demonstration of its correctness. Unfortunately, General Evans' literary talents are not equal to the military talents of his subject, and his work is not likely to add to the reputation of Field Marshal Slim.

The fifth and final volume of the official British history of *The War Against Japan*, by the late Major General S. Woodburn Kirby and others, begins with the final phase of the Burma campaign and preparations for the invasion of Malaya,

scheduled for September 1945 with Slim in command.* It then shifts scene to the Pacific to describe the operations of the American forces in the Indies and Okinawa, the air bombardment and naval blockade of Japan, US activities in China, Soviet entry into the war, and, finally, the surrender of Japan. But the volume does not end with the end of the war; in an additional eleven chapters the authors describe the postwar activities of Mountbatten's Southeast Asia command from August 1945 to November 1946, the liberation and reoccupation of Japanese-held territory, the recovery of prisoners of war and internees, and the beginnings of guerrilla warfare in Indonesia. A final section of four chapters provides an extremely useful overview of the entire war against Japan, including separate discussions of American strategy in the Pacific, combined strategy in Southeast Asia, and an epilogue summarizing the position of the Allies at the end of the hostilities. Like the other volumes in the British series, the concluding volume, though it lacks documentation, is beautifully printed and illustrated and contains a large number of excellent maps and charts and over thirty valuable appendixes.

The principal problem for the Allies in the period covered by this volume was how to persuade Japan, already defeated in the military sense, to capitulate. While plans and preparations for the invasion of the home islands, primarily an American effort, were being completed, the Allies, meeting in Potsdam, issued a declaration on July 27 offering Japan a choice between surrender or complete destruction. When the Japanese apparently rejected these terms, the first atomic bomb was dropped on Hiroshima on August 6, to be followed three days later by a second bomb on Nagasaki. It was only then, after the direct, unprecedented intervention of the emperor, that the Japanese surrendered. Since the present volume, as one would expect, is concerned mostly with the British effort against Japan, the authors have refrained from entering into the controversy over the American decision to use the atomic bomb, but they do provide a careful and accurate reconstruction of the struggle within the highest councils of the Japanese government before the decision to surrender was made. The writing on this subject is extensive, and the authors add nothing to what we already know.

The Allies, though they defeated Japan and won the war, gained little and lost much. China, which the United States had hoped to make the bulwark of the free world in the Far East, was lost to communism; and the former colonies, freed from Japanese domination, showed little inclination to return to their former status. "The most important results of the war against Japan," conclude the authors of the official British history, "were the emergence of Communist China as the dominant Power in East Asia in place of Japan, and the gradual disappearance of colonial rule. . . ." The Western powers, who had expected to be restored to their former position in the area taken from them by Japan, were ultimately driven from Southeast Asia. In a sense, the efforts of the United States today to re-establish Western influence in that region may be viewed as an effort to achieve what the Allies failed to achieve in World War II.

Unlike Korea or Vietnam, World War II was a popular war and for many who served in it the greatest experience of their lives. Now, from the comfortable distance of middle age, they can view that experience nostalgically through the eyes of others and perhaps learn the significance of the campaigns in which they fought. And, since many of today's problems were shaped by the events of 1939-45, they may perhaps also learn from a study of the war to understand better the nature of the world they made and their children now so bitterly seek to change.

BOOKS UNDER REVIEW

- THE CHAMPAGNE CAMPAIGN. By *Robert H. Adleman* and *George Walton*. (Boston: Little, Brown and Company. 1969. Pp. xiv, 298. \$7.95.)
- ROME FELL TODAY. By *Robert H. Adleman* and *George Walton*. (Boston: Little, Brown and Company. 1968. Pp. 336. \$7.95.)
- THE SECRET ROAD TO WORLD WAR TWO: SOVIET VERSUS WESTERN INTELLIGENCE, 1921-1939. By *Paul W. Blackstock*. (Chicago: Quadrangle Books. 1969. Pp. 384. \$9.50.)
- SALERNO TO CASSINO. By *Martin Blumenson*. [United States Army in World War II: The Mediterranean Theater of Operations.] (Washington, D. C.: Office of the Chief of Military History, United States Army. 1969. Pp. xvii, 491, 8 maps. \$9.50.)
- GERMANY'S MILITARY STRATEGY AND SPAIN IN WORLD WAR II. By *Charles B. Burdick*. ([Syracuse, N. Y.:] Syracuse University Press. 1968. Pp. xi, 228. \$7.00.)
- THE WAR IN THE FAR EAST, 1941-1945: A MILITARY HISTORY. By *Basil Collier*. (New York: William Morrow and Company. 1969. Pp. xiii, 530. \$8.95.)
- THE BITTER WOODS. THE DRAMATIC STORY, TOLD AT ALL ECHELONS—FROM SUPREME COMMAND TO SQUAD LEADER—OF THE CRISIS THAT SHOOK THE WESTERN COALITION: HITLER'S SURPRISE ARDENNES OFFENSIVE. By *John S. D. Eisenhower*. (New York: G. P. Putnam's Sons. 1969. Pp. 506. \$10.00.)
- VICTORY IN THE WEST. Volume II, THE DEFEAT OF GERMANY. By *L. F. Ellis*, with *A. E. Warhurst*. [History of the Second World War, United Kingdom Military Series.] (London: H. M. Stationery Office; distrib. by British Information Services, New York. 1968. Pp. xviii, 455. \$16.20 postpaid.)
- THE BATTLE FOR GERMANY. By *H. Essame*. (New York: Charles Scribner's Sons. 1969. Pp. xi, 228. \$7.95.)
- SLIM AS MILITARY COMMANDER. By *Geoffrey Evans*. ([Princeton, N. J.:] D. Van Nostrand Company. 1969. Pp. 239. \$8.95.)
- VICTORY AND OCCUPATION. By *Benis M. Frank* and *Henry I. Shaw, Jr.* [History of U.S. Marine Corps Operations in World War II, Volume v.] ([Washington, D. C.:] Historical Branch, G-3 Division, Headquarters, U.S. Marine Corps. 1968. Pp. xiii, 945. \$11.75.)
- KRIEG IM WESTEN: DIE HALTUNG DER HERRSCHENDEN KREISE DER USA UND GROSSBRITANNIENS ZUR POLITISCHEN UND MILITÄRISCHEN VORBEREITUNG DER ZWEITEN FRONT (1942 BIS 1944). By *Olaf Groehler*. ([Berlin:] Deutscher Militärverlag. [1968.] Pp. 278. DM 14.80.)
- SOFT UNDERBELLY: THE ANGLO-AMERICAN CONTROVERSY OVER THE ITALIAN CAMPAIGN, 1939-1945. By *Trumbull Higgins*. (New York: Macmillan Company. 1968. Pp. x, 275. \$6.95.)
- TO LOSE A BATTLE: FRANCE 1940. By *Alistair Horne*. (Boston: Little, Brown and Company. 1969. Pp. xxiv, 647. \$12.50.)
- THE MEDITERRANEAN STRATEGY IN THE SECOND WORLD WAR. By *Michael Howard*. (New York: Frederick A. Praeger. 1968. Pp. xii, 82. \$4.00.)
- THE BATTLE FOR ROME. By *W. G. F. Jackson*. (New York: Charles Scribner's Sons. 1969. Pp. vii, 224. \$7.95.)
- THE WAR AGAINST JAPAN. Volume V—THE SURRENDER OF JAPAN. By *S. Woodburn Kirby*, with *M. R. Roberts et al.* [History of the Second World War, United Kingdom Military Series.] (London: H.M. Stationery Office; distrib. by British Information Services, New York. 1969. Pp. xxiii, 599. \$22.68 postpaid.)
- CAPTAINS WITHOUT EYES: INTELLIGENCE FAILURES IN WORLD WAR II. By *Lyman B. Kirkpatrick, Jr.* ([New York:] Macmillan Company. 1969. Pp. xiv, 303. \$6.95.)
- ROMMEL AS MILITARY COMMANDER. By *Ronald Lewin*. ([Princeton, N. J.:] D. Van Nostrand Company. 1968. Pp. x, 262. \$8.95.)
- THE MIGHTY ENDEAVOR: AMERICAN ARMED FORCES IN THE EUROPEAN THEATER IN WORLD WAR II. By *Charles B. MacDonald*. (New York: Oxford University Press. 1969. Pp. 564. \$12.50.)
- LA SECONDE GUERRE MONDIALE. Volume I, LES SUCCÈS DE L'AXE (SEPTEMBRE 1939-JANVIER 1943). By *Henri Michel*. [Peuples et civilisations, Number 21.] (Paris: Presses Universitaires de France. 1968. Pp. vii, 505. 48 fr.)
- ON BORROWED TIME: HOW WORLD WAR II BEGAN. By *Leonard Mosley*. (New York: Random House. 1969. Pp. xvi, 509. \$8.95.)

- THE FAST CARRIERS: THE FORGING OF AN AIR NAVY. By *Clark G. Reynolds*. (New York: McGraw-Hill Book Company. 1968. Pp. xvi, 498. \$12.50.)
- THE 900 DAYS: THE SIEGE OF LENINGRAD. By *Harrison E. Salisbury*. (New York: Harper and Row. 1969. Pp. xi, 635. \$10.00.)
- ERINNERUNGEN UND GEDANKEN. By *Georgi K. Schukow*. (Stuttgart: Deutsche Verlags-Anstalt. 1969. Pp. 692. DM 28.) *See also* Zhukov, Georgi K., *below*.
- THE BOMBER OFFENSIVE. By *Anthony Verrier*. ([New York:] Macmillan Company. 1969. Pp. x, 373. \$8.95.)
- IRON COFFINS: A PERSONAL ACCOUNT OF THE GERMAN U-BOAT BATTLES OF WORLD WAR II. By *Herbert A. Werner*. (New York: Holt, Rinehart and Winston. 1969. Pp. xviii, 329. \$7.95.)
- THE FIRST SUMMIT: ROOSEVELT AND CHURCHILL AT PLACENTIA BAY 1941. By *Theodore A. Wilson*. (Boston: Houghton Mifflin Company. 1969. Pp. xvi, 344. \$7.50.)
- MARSHAL ZHUKOV'S GREATEST BATTLES. By *Georgi K. Zhukov*. Edited with an introduction and explanatory comments by *Harrison E. Salisbury*. Translated from the Russian by *Theodore Shabad*. (New York: Harper and Row. 1969. Pp. 304. \$6.95.) *See also* Schukow, Georgi K., *above*.

* * * * * *Reviews of Books* * * * * *

General

HAMLET'S MILL. AN ESSAY ON MYTH AND THE FRAME OF TIME. By *Giorgio de Santillana* and *Hertha von Dechend*. (Boston: Gambit. 1969. Pp. xxv, 505. \$10.00.)

Two notable historians of science have produced a book of twenty-five chapters, emblazoned with sibylline epigraphs (typically "*Tout-puissants étrangers, inévitables astres*," from Paul Valéry's *Jeune Parque*, or "History is a nightmare from which I am trying to awake," from James Joyce's *Ulysses*), and fortified with thirty-nine appendixes, a bibliography, and indexes. It is a "self-styled first reconnaissance over uncharted ground" (p. 344), set to prove that world-wide mythology is reducible to a monomyth about "cosmological time," originated some millennia ago by "some almost unbelievable Near Eastern ancestor who first dared to understand the world as created according to number, measure and weight" (p. 132). Thus myth is the repository of arcane astronomical (and astrological) knowledge, and the task is to free the genius of that neolithic Einstein from the overlays of subsequent obscurity and obfuscation. This the authors do by a combination of heavy scholarship, withering hubristic polemicism, and portentous oracular style. The cowed reviewer is soon reduced to wondering whether mere critical prose should even be expended on something that obviously solicits the suspension of disbelief.

Attempting nevertheless to apply customary criteria, one runs into classificatory difficulties. Is this history of science, or philosophy, or folk psychology, or . . . ? Whatever it may be, the authors have a clear idea of who is incompetent to review it, judging from De Santillana's indignant rejoinder to an anthropologist's strictures (Edmund Leach in *The New York Review*, XIV [Feb. 12, 1970]). The blurb prefers encomiastic endorsements ("here is a book for the wise") or grovelling proskynesis ("I was ignorant, and this work has . . . ground me to smithereens"). Whatever the work's merits as a guide to cosmographic oddments from many eras and climes (interspersed with an occasional "Guide for the Perplexed"), or as a collection of yarns from Saxo Grammaticus, Snorri Sturluson ("Amlodhi's mill" as a kenning for the sea!), Firdausi, Plato, Plutarch, the *Kalevala*, *Mahabharata*, and *Gilgamesh*, not to forget Africa, the Americas, and Oceania, it does attempt to explain at one stroke all of man's myth and must thus be held accountable as a treatise on mythology. On such a level it is even more astounding than in its shattering overall tenets. The authors pay homage to "the preternaturally perceptive minds" of Athanasius Kircher (the seventeenth-century Jesuit) and Charles Dupuis (who in 1795 declared that myth originates in science and must be explained accordingly). More modern heroes are Leo Frobenius (von Dechend's teacher) and Marcel Griaule (specialist on West African cosmology). There is a sometimes patronizing awareness of Mannhardt and the Cambridge ritualists and a biting hostility toward evolutionism and psychoanalytic myth interpretation. The long-forgotten period-piece etymologies of Max Müller and Adalbert Kuhn ("surely a great scholar," p. 381) are blithely resurrected (for example, Sanskrit *Pramantha* matching Greek *Prometheus*, p.

139), while more up-to-date authorities are caricatured as "severe philologists, slaves to exact 'truth'" (p. 294). In the epilogue (pp. 326–28), Ernst Cassirer takes a gratuitous beating as "blinded by condescension" and reaping "the wages of the sin of intellectual pride." And this from those who in "solving" myth accord the silent treatment to Durkheim, Jung, Eliade, Dumézil, Lévi-Strauss, and many others!

In brief, this is not a serious scholarly work on the problem of myth in the closing decades of the twentieth century. There are frequent flashes of insight, for example, on the cyclical world views of the ancients and on the nature of mythical language, as well as genuinely eloquent, quasi-poetic homilies. As a whole, however, these mystagogic ministrations are at best some kind of recessionary for the expiring Age of Pisces (inaugurated by "Christ the Fish" in the time of Augustus), and at the same time a precessional for the equinoctially induced new Age of Aquarius. Possibly for this new age the work will prove a true enchiridion to cosmic sensitivity training. This reviewer, a pre-Aquarian Aquarius, chooses to remain a "slave to exact truth"; *sapienti sat*.

University of California, Los Angeles

JAAN PUHVEL

THE RENAISSANCE DISCOVERY OF CLASSICAL ANTIQUITY. By *Roberto Weiss*. (New York: Humanities Press. 1969. Pp. xii, 222. \$8.50.)

THIS last book by the late Roberto Weiss is a pioneering effort. Rich in detail and written at times with the concision of a handbook, this is the first general treatment of the early history of the study of archeology as it developed in Italy in the period from the early fourteenth century down to the Sack of Rome in 1527. In contrast to the general medieval attitude, which valued the tangible remains of the ancient world largely because these objects were attractive or useful, in increasing numbers men in the fourteenth and fifteenth centuries came to cherish them precisely because they were ancient. Intimately connected with this desire to study antiquities were the rise of a sense of historical perspective and the sharpening of a critical faculty aimed at sorting fable from truth.

The opening chapters of the book present historically the evolution of the enthusiasm for antiquities down to the opening decades of the fifteenth century. It was the task of Alberti and Biondo to establish the basic methodology for a study that, up to their time, had been pursued unsystematically. Because by 1450 antiquarian interests were becoming so diversified, in the remainder of his work the author turns to a topical treatment sketching chronologically the developments in each area. Two chapters are given to the study of the antiquities and topography of the ancient city of Rome and two to antiquarian research pertaining to other parts of Italy. There are also chapters on Greek archeology, epigraphy, numismatics, and archeological collections.

For Weiss the achievements of Biondo in the mid-fifteenth century were not superseded for a hundred years. The decades after his death rather should be considered primarily as a time for consolidation of methodology by lesser men. In view of the Italian-wide scope of archeological investigation by the sixteenth century, however, the reader wonders why the termination date of 1527 was chosen. Bembo (p. 202) and Fulvio (p. 89) are said to have ended an era and the Sack of Rome to have initiated "a new consciousness" leading to "a new archaeology" (p. 89), but the author tantalizingly says nothing more. A few words here might have given more perspective to an otherwise excellent book.

Harvard University

RONALD WITT

THE ATLANTIC SLAVE TRADE: A CENSUS. By *Philip D. Curtin*. (Madison: University of Wisconsin Press. 1969. Pp. xix, 338. \$7.50.)

PROFESSOR Curtin's new book is up to his customary standard of performance: within the limits he set for himself, *The Atlantic Slave Trade* could hardly be a better or more important book. It "seeks to explore old knowledge, not to present new information." Curtin has carefully sifted through the enormous body of printed material—but deliberately excluded untapped archival material—in order to determine how many people came as slaves from Africa and when and where they came from and went to. He has, in so doing, called a halt to the practice of tossing about dubious estimates.

Since the data from various sources are not parallel, Curtin had an exasperating task in trying to line up the more useful statistics. As a result, Curtin's own figures are necessarily rough, and he warns against using them as more than a point of departure. In fact, the book is rendered especially useful for students by Curtin's running criticism of his own methods and assumptions and of the data themselves.

The book begins with a survey of the literature and the "numbers game" contained therein. It proceeds to two chapters on spatial distribution and one on temporal. Thereafter come three more chapters on the trade during the eighteenth century, one on the nineteenth, and a final chapter on major trends. We are then offered a postscript on mortality, which ought to lay to rest a good deal of propagandistic nonsense, and an appendix on Koelle's Linguistic Inventory.

Although Curtin largely limited himself to the task of counting, he does indicate the wide range of social implications in his new census. One example may suffice. The United States received only five per cent of all Africans in the trans-Atlantic trade; yet in 1950 it contained more than thirty per cent of all those of African descent in the Western Hemisphere. What happens, then, to the common assumption that Southern slavery was so much more rigorous and brutal than Caribbean or South American? Reproduction being a major index of treatment, something is seriously wrong. Curtin does not explore fully this and similar questions in his book, but he does open them for discussion and provides several stimulating observations.

The great contribution of Curtin's work lies in its careful, historically informed assessment of what we now can responsibly say. It should stimulate new research by providing an adequate guide and framework and by removing a great many misconceptions. *The Atlantic Slave Trade* raises the discussion to a much higher level, sets impressive standards for those who may wish to extend Curtin's analysis or to challenge his findings, and provides a standard against which to measure future work.

University of Rochester

EUGENE D. GENOVESE

THE EMERGING NATIONS AND THE AMERICAN REVOLUTION. By *Richard B. Morris*. (New York: Harper and Row. 1970. Pp. xiii, 238. \$6.95.)

THIS book is concerned not only with the influence of the American Revolution on the emerging nations, as indicated by the title, but also with American foreign policy toward them. Professor Morris recalls that the American Revolution, like those in the new states, had its declaration of independence, its war of national liberation, its guerrilla tactics, its mass demonstrations against a foreign oppressor, and its crisis of legitimacy. The American revolutionaries welcomed foreign aid, but they adopted a neutralist policy once victory had been won and refused to honor prewar debts. Professor Morris also discusses in considerable detail the extent of American assistance to the liberation of other peoples in Latin America, Asia, and Africa from colonial rule.

In treating this subject Professor Morris writes with the enthusiasm of an advocate rather than the dispassion of a judge, and seeks to reply to those who view the United States as imperialist, racist, and anti-Populist. The American policy is discussed in terms of a struggle between "the free world" and "totalitarian societies," and the narrative is buoyed by a confidence that most Americans have already achieved the free and abundant life heralded by Jefferson's "pursuit of happiness." The comparison of the American and later revolutions tends to stress analogies at the expense of contrasts, and the account of American foreign policy is little concerned with cases where it has supported the status quo against threats of revolution. The cultural relativism reflected in his conclusion, which urges the employment of ideas rather than force and recognizes diverse forms of democracy, does not therefore strike the reader as fully congruent with the predominantly ethnocentric tone of much of the preceding text.

Princeton University

CYRIL E. BLACK

THE CITY IN AMERICAN HISTORY. By *Blake McKelvey*. [Historical Problems: Studies and Documents, Number 9.] (New York: Barnes and Noble. 1969. Pp. xii, 16-229. \$5.75.)

NINETEENTH-CENTURY CITIES: ESSAYS IN THE NEW URBAN HISTORY. Edited by *Stephan Thernstrom* and *Richard Sennett*. [Yale Studies of the City, Number 1.] (New Haven, Conn.: Yale University Press. 1969. Pp. xiii, 430. Cloth \$12.50, paper \$4.95.)

TWENTY years ago, urban history counted fewer than a half-dozen professional practitioners and was taught regularly at no American university. In recent years, as graduate students and young scholars in increasing numbers have come to regard urbanization as the central fact in American history, the field has entered a state of creative ferment. There is no clear distinction between the "old" and the "new" urban historians, but many members of the latter group tend to have a narrow focus, an empirical approach, and a strong desire to provide "a more reliable description of the experience of most Americans than can be gleaned from the impressions of an articulate minority, whether fellow citizens or foreign observers."

The City in American History is not advertised as "old" or even traditional urban history, but its author has been writing in the field longer than most of the "new" researchers have been alive. City historian of Rochester, New York, since 1936, Blake McKelvey was a founder of the Urban History Group and has been the most prolific of its early members. Unfortunately, this is the least successful of his eight major books. The 110-page essay, which introduces a standard collection of documents of about equal length, is far less interpretive than might have been expected from such an experienced scholar in such a slender volume. Instead, the prose is overburdened with detail and represents in large measure simply a distillation of McKelvey's much longer work on urbanization since the Civil War.

Nineteenth-Century Cities, on the other hand, is an initial offering of the "new" urban history. It consists of twelve papers, together with an afterword by Norman Birnbaum, that were presented in November 1968 at the Yale Conference of the Nineteenth Century Industrial City. According to the co-editors Stephan Thernstrom and Richard Sennett, the conferees generally shared three traits: (1) an interest in linking sociological theory to historical data, (2) an understanding of the use of quantitative materials, and (3) a desire to study the social experience of unexceptional people.

Space does not permit arguing the merits of all the articles, which are organized

loosely into sections on class and mobility patterns, residential patterns, elites and political control, and families. With the exception of a long piece by Anthony Maingot on "Civil-Military Conflict in Urban Colombia," the various essays complement each other more than is normal in such books. Outstanding among them is Jean Scott's, "The Glassworkers of Carmaux, 1850-1900," which tests the hypothesis common among nineteenth-century police and modern sociologists that working-class consciousness was associated with rootlessness and migrant status. Her findings suggest the opposite; the glassworkers became more militant as their attachment to craft diminished and their attachment to place increased. Particularly interesting is her comparison of glassworkers with miners, who also were undergoing occupational displacement but who had a historically different orientation to the town.

Methodologically, the articles vary considerably, even when dealing with somewhat similar questions. Herbert Gutman's account of the blue-collar social origins of locomotive, iron, and machinery manufacturers in Paterson, New Jersey, is based upon a complete analysis of a particular subgroup of the population. Stephan Thernstrom's, "Ethnic Differences in Occupational Mobility in Boston, 1890-1940," Stuart Blumin's "Mobility and Change in Ante-Bellum Philadelphia," and Peter Knights's "Population Turnover, Persistence, and Residential Mobility in Boston, 1830-1860," on the other hand, rely upon samples taken from the entire population of a city, while Clyde Griffin and Michael Katz in their studies of Poughkeepsie, New York, and Hamilton, Ontario, actually offer analyses developed from a study of the entire adult male population.

The conclusions emerging from such enormous effort—such as the poor were more numerous and more mobile than the rich, and the young moved more often than the old—will not surprise many readers; their importance rests in their analysis of the degree to which such factors as ethnicity, age, and occupation affected mobility. For instance, Thernstrom suggests that differences among ethnic groups were as significant as those between foreign- and native-born, and Knights finds that residential mobility was twice as high in Boston before the Civil War as it is in the United States today. Particularly surprising is Blumin's conclusion that by 1860 in Philadelphia lower-ranked occupations and neighborhoods were expanding faster than the population as a whole. His use of quantitative materials is imaginative and skillful, but unconvincing. For instance, his analysis regards any residential move toward the distant periphery between 1820 and 1860 as representing downward mobility. This may have been true for the earlier years, but this is a highly dubious assumption for 1860.

Inevitably, many of the essays suffer from the problem of the representativeness and accuracy of the sources and from the general difficulty of reducing human experience to quantitative expression. But *Nineteenth-Century Cities* presents two problems not endemic to the "new" urban history. With few exceptions, the writing is cumbersome and dull and unlikely to attract a broad readership. While this results partially from the empirical and narrow focus of most of the studies, it is apparent that many of the authors are more concerned with the methodology than the prose. Secondly, these essays do not exhibit the care that they purportedly seek. The tables frequently fail to include descriptive titles or sources. Especially noticeable is the failure to justify sample sizes by statistical or any other means or to explain precisely how the samples were drawn. As they publish their larger studies, I hope these authors will provide more precise information about their methods of data collection.

In brief, the quantitative methods of *Nineteenth-Century Cities* are not sufficiently sophisticated or rigorous to impress statisticians or econometricians. At the same time, the empirical orientation and bland style of many of the essays will not endear them to

traditional historians. But this is an important book, well meriting the attention of anyone concerned with social and urban history. It highlights a promising and exciting method of inquiry and possibly provides "a foreshadowing of the direction in which the field will develop in the future."

Columbia University

KENNETH T. JACKSON

ITALIA E STATI UNITI NELL'ETÀ DEL RISORGIMENTO E DELLA GUERRA CIVILE: ATTI DEL II SYMPOSIUM DI STUDI AMERICANI, FIRENZE, 27-29 MAGGIO 1966. [Università degli Studi di Firenze, Pubblicazioni dell'Istituto di Studi Americani, Number 2.] (Florence: "La Nuova Italia" Editrice. 1969. Pp. 401. L. 3,500.)

STUDENTS and scholars on Italo-American cultural and diplomatic relations owe a special debt of gratitude to the United States Information Service, the Commune of Florence, the Provincial Bureau of Tourism, the Autonomous Office of Tourism, and the publisher, La Nuova Italia, for providing the necessary funds that made possible the publication of this volume. It contains the full text of the papers presented at the Second Symposium on American Studies held in Florence in May 1966, under the sponsorship of the Institute of American Studies of the Teachers College Faculty of the University of Florence. The participants at the symposium included Italian, American, and other historians well known for their scholarly contributions on the subject of Italo-American relations.

The volume opens with a brief introductory statement by Professor Agostino Lombardo on the cultural relations between Italy and the United States during the Risorgimento and the American Civil War. Four papers deal with the general topic of American writers prior to that period: Elémire Zolla on Melville, Aldo Celli on "The Risorgimento in American Poetry," Rolando Anzilotti on Henry Theodore Tuckerman, and A. William Salomone on "The Risorgimento in American Historiography."

The political relations between Italy and the United States are discussed by Giorgio Spini, dealing with politics, John Manigaulte on diplomacy, Gaetano Arfe in connection with Giuseppe Bertinatti, and Raimondo Luraghi, who writes on the technological, logistic, and tactical and strategic revolution in the two wars.

American influences on Italy form the subject of four papers. Joseph Rossi examines the role of the "American myth" in the political thought of the Risorgimento; Salvo Mastellone discusses the views of the men of the Risorgimento on the Constitution of the United States; James Woodress investigates the reception of *Uncle Tom's Cabin* in Italy; and Enzo Tagliacozzo contributes an essay on "Lincoln and the Risorgimento." The subjects of Americans in Italy and the Italians in America are discussed in three papers by Biancamaria Tedeschini Lalli on Francesco Varvaro Pojero, Claudio Gorlier on Samuel F. B. Morse, and Sergio Perosa on American novelists in Venice.

So much for the contents of this interesting volume. However, in order to appreciate more fully the subject of the symposium, it must be remembered that decades before the era of the Risorgimento and the American Civil War, many Americans, among them Franklin, Irving, Cooper, and Emerson, to mention only a few, were already well known in Italy. In those years the Italians viewed the United States not only as a literary and social myth, but more particularly as a political model, which they might profitably study. As proof of this interest one needs only to refer to the numerous statements made by Mazzini and other Italians on the role of the United States in Europe, and to the

many Italian exiles—Maroncelli, Foresti, Confalonieri, Garibaldi, and hundreds of others—who sought political asylum in this country.

This subject on Italo-American relations is so vast that it was not possible at this symposium to cover adequately and more fully many of its facets: the diplomatic relations between the United States and the Kingdom of the Two Sicilies; the diplomatic relations between the United States and the papal states; the role of Mazzini and his republican followers on the relations between Italy and the United States; the role of the Italians in the United States on the Risorgimento; Lincoln's Italian volunteers from New York; volunteers from Italy for Lincoln's army; Canadian and American Zouaves in the papal army; official relations between the Grand Duchy of Tuscany and the United States; United States recognition of the Kingdom of Italy; and so on. But these are subjects that Italian scholars may well take into consideration in planning the next symposium.

Columbia University

HOWARD R. MARRARO

THE ROMAN YEARS OF MARGARET FULLER: A BIOGRAPHY. By *Joseph Jay Deiss*. (New York: Thomas Y. Crowell and Company. 1969. Pp. xiii, 338. \$6.95.)

THIS biography is the most complete record yet published of those years, 1846–50, when Margaret Fuller traveled in Europe and lived in Italy. Mr. Deiss sketches briefly Margaret Fuller's early life and devotes most of his book to those years in Europe, primarily Italy, that have remained obscure until now.

On August 11, 1846, the *Cambria* docked at Liverpool, bringing Margaret Fuller, in the company of her traveling companions Rebecca and Marcus Spring, to England on the fastest Atlantic crossing made up to that time. Miss Fuller visited several cities and paid personal visits to Wordsworth and Carlyle. In Paris she visited George Sand and heard Chopin play. But it was in Italy—especially Rome—that Margaret felt really at home. In St. Peter's Cathedral on Easter Sunday 1847, Margaret met Giovanni Ossoli, who became first a friend and then her lover and the father of her child.

Liberally quoting from letters between Margaret and Giovanni, Mr. Deiss presents their growing friendship in their own words. The entire book is written in an imaginative, descriptive style that allows people and places and events to come vividly before the reader. Margaret recounted many of her experiences in letters written home to Horace Greeley and published in his New York *Tribune*, now collected in *At Home and Abroad* (Boston, 1856), edited by Arthur B. Fuller. Mr. Deiss has drawn on this collection as well as other published sources and many previously unpublished and untranslated letters for the materials for his narrative.

The personal lives of Margaret Fuller and Giovanni Ossoli are inseparably linked with the turbulent political events in Italy in 1848–49: suppression in the north by Austria, the conflict over the temporal power of the pope, the declaration of a constitutional republic in Rome in 1849, the opposition and flight of the pope, and finally the arrival outside Rome of the French forces, which attacked the city until it fell. Margaret Fuller definitely sympathized with the people in their desire for a democratic republic, but the forces of traditional aristocracy, absolutism, and papal authority triumphed.

Giovanni Ossoli was a member of an ancient, aristocratic, Catholic family in Rome that traditionally served the pope. It would be difficult to think of a more "unsuitable" match than that between him and Margaret Fuller, an American Protestant with decided democratic political persuasions. But Giovanni was a member of the National Guard, which fought for the Roman Republic, and they shared the dream of freedom

under a constitution for their beloved Rome. In his book, Mr. Deiss has added immeasurably to the knowledge of the life and character of Giovanni Ossoli.

Mr. Deiss's narrative describes fully the hardships and excitement of the weeks of the struggle and the final siege, with all its many participants. Margaret Fuller knew both Giuseppe Mazzini and Adam Mickiewicz, who played vital roles in the defense of Rome's short-lived republic. Not even Garibaldi and his legion could save this premature effort. Margaret gave encouragement to the embattled city and its leaders wherever she could, and, at the request of her friend Princess Belgiojoso, she served in a practical way in a hospital caring for the wounded.

Disinherited and disowned by his family after the failure of the revolution, Giovanni Ossoli fled with his wife and infant son to Florence, where they lived until May 1850 in a circle of friends including American artists and Elizabeth and Robert Browning. Margaret enjoyed family life and spent some time each day writing a history of the Roman revolution from her notes and journal entries. In order to support the family with her writing, Margaret decided to return with them to America, where she hoped to sell her manuscript on the revolution. The voyage was ill-omened. The small merchant ship on which they sailed ran aground and sank off Fire Island, New York, in mid-July 1850, and the lives of Margaret, Giovanni, and their little boy Angelo were lost.

Mr. Deiss has been able to find nothing new on the marriage of Ossoli and Margaret Fuller. He repeats the report Margaret gave a friend that they were married in December 1848 and writes that in the summer of 1849 "the topic of marriage was again placed on the agenda for active discussion, but no record exists that anything was yet done about it" (p. 280). He disregards evidence published in Madeleine Stern's biography that the marriage took place in April 1849.

Mr. Deiss, vice director of the American Academy in Rome, has written a much-needed book and written it well. His knowledge of Italian and of Rome, of Margaret Fuller's life and writings, and of the political history of the times has enabled him to write a lively yet accurate account of this important chapter in the final unification of Italy. He shows what an accurate observer Margaret Fuller was and how valuable as a historical record her book on the Roman revolution would have been. Unfortunately it was lost in the shipwreck. In his bibliography Mr. Deiss shows himself familiar with all the major writings of Margaret Fuller and many of the critical and biographical works about her. Although the form of the footnotes is annoying in its imprecision and incompleteness, the book will be of interest to the scholar as well as to the general reader.

University of Wisconsin, Milwaukee

RUSSELL E. DURNING

THE IDEOLOGY OF FASCISM: THE RATIONALE OF TOTALITARIANISM.

By *A. James Gregor*. (New York: Free Press. 1969. Pp. xv, 493. \$11.95.)

EUROPEAN FASCISM. Edited by *S. J. Woolf*. [Reading University Studies on Contemporary Europe. I, Studies in Fascism.] (Reprint; New York: Vintage Books. 1969. Pp. 387. \$2.45.)

SINCE the mid-1960's, with the publication of Eugen Weber's *Varieties of Fascism* (1964) and the issue on "International Fascism," in the *Journal of Contemporary History* (I [1966]), the revival of interest in this subject has produced works of the most disparate kinds. Of the two books under review here, the second is a collection of historical essays based on a series of lectures at the University of Reading; it covers twelve European countries individually during the interwar period and includes general observations about European fascism by S. J. Woolf, H. R. Trevor-Roper, and Christo-

pher Seton-Watson. The first book, by a Berkeley political scientist, describes only the ideology of Italian fascism, but it then uses this description to suggest a general typology for all totalitarian movements in all parts of the world throughout the twentieth century. Thus, the two books illustrate the tendency of most historians to see fascism as a phenomenon of a particular time and place and the tendency of most political scientists to see it as a “model” whose manifestations can recur anywhere—though few have gone as far as Gregor in this respect.

According to Gregor, Italian fascism was the “paradigm” of all other “totalitarian” movements—from Stalinism to current African “national socialisms.” In order to support this startling assertion he tries to combine insights from theories of totalitarianism, modernization, and the sociology of ideologies. His effort is impressive, but his overall argument is weakened by the fact that the jargons of these theories mix badly together and are sometimes confusing even separately. “Totalitarian,” for example, has become simply a “boo” word to liberals, like “fascist” to radicals and “communist” to conservatives. Surely today the questionable concept of totalitarianism should be replaced with the more empirical and less loaded concept of mobilization. Fascism may or may not have been a modernizing ideology, but, in any case, it was directed primarily toward the integration of society—political and social mobilization—rather than economic mobilization. Gregor misreads the whole temperament and outlook of Mussolini by seeing him as a “productionist,” merely on the basis of a few of the Duce’s thousands of newspaper articles. He is on much sounder ground when he asserts that fascist and quasi-fascist movements strive for “the rapid attainment of status for status-deprived national communities.” Still, one is troubled to see many regimes in Africa called “fascist” because of their emphasis on “the people” and their insistence on the dominant role of the state. By this standard Robespierre was also a fascist, as is Fidel Castro, whom Gregor does not even mention.

Gregor gives the most comprehensive description available of Italian Fascist ideology. Its three principal sources were the antiparliamentarian sociological tradition of Gumpowicz, Mosca, and Pareto, the radical syndicalist tradition of Sorel, and the nationalist tradition of Corradini. The unifying concept that articulated these elements into a defensible rationale was the Gentilean notion of the ethical state. Gregor insists that Mussolini’s famous article on fascism in the 1932 edition of the *Enciclopedia Italiana* was the basis of the official philosophy of fascism, as well as its political and social doctrine. He also argues that, just as the social and political philosophy of Marxism was the product of the genius of Karl Marx, so that of fascism was the product of the genius of Giovanni Gentile, whereas Marxism and fascism as doctrines were the products of many hands. Indeed, he views Mussolini as an Italian Lenin, striving to adapt his Marxist background on both the ideological and practical levels to the new conditions created by the First World War and its aftermath. According to Gregor, the Mussolinian-Gentilean ideology of 1932 was completed by an indigenous fascist racial doctrine (contaminated by Nazi doctrines but still distinct) and the “socialization” program of the ephemeral Republic of Salò in 1944–45.

Although Gregor’s analysis produces the desired “paradigm” of totalitarian movements, it unfortunately distorts Fascist ideology almost as much as the Marxist oversimplifications the author so skillfully demolishes. First of all, it leaves out the whole militaristic, *squadristi* ingredient of early fascism; the “ideology” of Farinacci and Balbo was more anarchic than totalitarian but just as fascist as Mussolini’s. Second, Gregor minimizes Rocco’s contribution to the fascist philosophy of the state, which was more authoritarian than totalitarian but just as fascist as Gentile’s. A third though lesser

misrepresentation is the description of Ugo Spirito as Gentile's "foremost philosophic heir" (pp. 360-61). Spirito was a kind of humanistic communist, whereas Gentile was an authoritarian nationalist; lumping these two antagonists together ideologically is as unjustifiable as lumping Marx and Hegel together ideologically. Despite these distortions, however, Gregor has performed an important service in showing that Italian fascism had an ideology, and he has set forth a thought-provoking thesis about its applicability in other "developing" nations.

Unlike Gregor, most of the contributors to *European Fascism* view fascism as a reactionary rather than a revolutionary phenomenon. They consider German, Italian, and French fascists as very much in league with big capitalism and Hungarian and Rumanian fascists as exponents of a kind of intellectualized peasant archaism. Yet there is a common theme of declassed intellectuals leading students and people on the margins of the lower-middle and lower classes in an effort to wrest power from the existing establishment, be it liberal or conservative. Organized violence, emotionalism, and demagoguery are also listed as typical of European fascism between the two world wars, though this combination of traits has been adopted by other groups of malcontents since then. The prestige of fascism was so great during the 1930's that reactionary nationalist regimes like Franco's Spain and Salazar's Portugal took on many of its trappings: glorification of the leader, corporativism, militias in colored shirts. We are thus reminded of the more traditional meaning of the word "model." As in all collections of this type, the essays in this volume vary in quality, but all are useful, and some are based on personal experiences as well as reflective scholarship.

New York University

EDWARD R. TANNENBAUM

CRUSADE OF THE LEFT: THE LINCOLN BATTALION IN THE SPANISH CIVIL WAR. By Robert A. Rosenstone. (New York: Pegasus. 1969. Pp. xix, 21-415. \$8.95.)

ROSENSTONE's monograph is the fourth full-length study of the American volunteers who fought in the Spanish Civil War and the first to move beyond Edwin Rolfe's *The Lincoln Battalion* (1939). Rolfe's book remains unrivaled in its understated eloquence and in its narrative simplicity, which is no surprise when one remembers that Rolfe was a gifted poet whose work has only recently been praised (and anthologized), but Rosenstone goes beyond Rolfe, and far beyond Cecil Eby and Arthur H. Landis, in his ability to remain sympathetic to the volunteers without polemical distortions of political and psychological reality. Rosenstone acknowledges that most of the volunteers were attracted by "journalistic Marxism" and admits that many, if not most, were members of the Communist party; but his familiarity with the 1930's enables him to place commitments in context and to pass judgment with compassion. He shows that some commissars in Spain misused their power, but he recognizes that men like Steve Nelson and John Gates played a necessary role in a war that was intensely ideological. The propaganda spewed out by the *Daily Worker* was scorned even by the men in the trenches, but Rosenstone understands that the Communists did what the other anti-Fascists were unable to do: they brought 3,000 Americans (and over 40,000 others) to Spain. For the wild assertions of J. Edgar Hoover and HUAC, Rosenstone has brief, ironic refutations. In addition to his restrained and reasonable account of political factors, Rosenstone provides statistical data not previously available, on the occupations, ethnic backgrounds, states of origin, and ages of the volunteers. There are few surprises. We have known all along that the National Maritime Union contributed a large number

of men and that Jews were disproportionately represented, but Rosenstone's figures are important corroborations of sometimes impressionistic evidence. Despite a sprinkle of men from wealthy families, the majority of those who went to Spain were what the Communist party claimed they were—workers. That Negroes were probably fewer than claimed is no surprise; the party was never as successful with Negroes as it hoped to be. Although specialists will probably be most interested in these collections of sociological data and in Rosenstone's comments on the alleged "terror" within the Lincoln Battalion, most of *Crusade of the Left* is narrative history, the story of men at war, of their hardships, enthusiasm, demoralization, and, finally, their satisfaction that they had done their part. It is not clear whether the author or the publisher is to blame, but every map in the book is taken, without acknowledgment, from Rolfe's history.

Amherst College

ALLEN GUTTMANN

Ancient

ESSAYS ON THE GREEK HISTORIANS AND GREEK HISTORY. By *H. D. Westlake*. (New York: Barnes and Noble. 1969. Pp. 332. \$9.50.)

THE HISTORIANS OF GREECE AND ROME. By *Stephen Usher*. (New York: Taplinger Publishing Company. 1970. Pp. xi, 273. \$6.50.)

WESTLAKE's book of eighteen essays emphasizes Thucydides and the Peloponnesian War, with a secondary stress on Greek Sicily, the two subjects being tied together by the essay on Hermocrates, who, in addition to taking part in the Peloponnesian War, is also held to have pointed ahead to Dionysius and fourth-century events in Sicily. These events round out the volume in a series of five essays. The eighteenth title, "Eumenes of Cardia," excellent though it is, lies outside the main theme of the work and therefore will not be discussed here.

The lead essay on "Irrelevant notes and minor excursions in Thucydides" ingeniously argues that these digressions show Herodotean influence, while later Thucydides sloughs this off, sacrificing all details that do not contribute directly to his main purpose. In support of his contention the author shows that there is less of this irrelevant material in the second part of the *History* than in the first, from which he concludes that Thucydides changed his approach to the writing of history. The examples Westlake gives of Herodotean digressions are quite persuasive when examined as a group, but their relative scarcity admits of more than one explanation, even if we assume—and it is somewhat hazardous to do so—that we can separate Thucydides' early and late periods. Certain natural phenomena, such as the eruption of Mount Aetna or an earthquake followed by tidal waves, would not have been repeated by Herodotus himself, who also has fewer digressions in the later part of his work; thus, Thucydides' cryptic references to earthquakes in the later books might have been anticipated. They need not mean a change in the historian as Westlake infers. On the other hand Westlake has no patience with the statement made in antiquity by Cratippus that Thucydides decided not to use speeches in the last part of his *History*; yet was not Cratippus in his own way coming to grips with the modern question about that historian's development?

Other points made by Westlake include some questions about Thucydides' impartiality, particularly in his account of the Thracian campaign in which he was judged by the Athenian government to have been remiss. The author explains certain inadequacies in the narrative by Thucydides' ignorance of the facts. For example, he

failed to understand Pericles' campaign strategy because he had not been one of the board of generals under him. When challenged by Gomme, however, Westlake changes his ground. The account of Pericles' offensive operations is inadequate because Thucydides regarded them as unimportant. The essay on Xenophon's *Hellenica* is less penetrating. Xenophon is charged with failing to do justice to Lysander and to Epaminondas because he lacked an eye for greatness. This same Xenophon recognized that quality in Socrates when other Athenians clearly failed to do so.

Usher's book deals primarily with four Greek historians (Herodotus, Thucydides, Xenophon, and Polybius) and three Romans (Sallust, Livy, and Tacitus), though a final chapter offers useful information on minor figures like Diodorus Siculus and Appian, whose chief importance is in having outlasted their rivals.

In such a general work it is hard to avoid dogmatism; certainly Usher has not done so. He speaks of Herodotus' friendship with Sophocles as though it were an established fact, and he fails to deal with the question of how Herodotus developed into the first historian. He tentatively accepts the view that Thucydides wrote his narrative of events first and only later turned back to insert the speeches. He boldly states that Alcibiades had the ability to win in Sicily, a far less balanced estimate of Alcibiades than we find in Westlake. But in dealing with Xenophon, Usher has something fresh to offer, even though not everyone will see Xenophon as "the victim of an intensifying left-wing campaign that happened to coincide with his return to the Greek world." On Polybius, understandably, he relies a good deal on Pédech, while his account of Sallust owes much to Syme. Livy's accomplishment in writing a monumental history is brought home to us when Usher proclaims it as equivalent to writing a 300-page volume every year for forty years! Livy's mistakes in Greek (translating "shields" as "doors" in an account of a battle) and his over-rhetorical style are mentioned on the debit side, but Usher acquits him of the unfair charge of writing propaganda for Augustus. The thirty-five pages on Tacitus add nothing particularly new, perhaps too much to ask about a writer who has been so much studied of late.

Westlake's essays will continue to interest the specialist, but they also offer much to any reader who is not in too much of a hurry. Usher frequently displays a gift for turning a phrase, but he is not always judicious. But no one who writes such a general book can hope to satisfy his colleagues! Those who read it will wish to read the historians at first hand, and when they do they will judge Usher's book for themselves. Westlake's notes are a model of accuracy, but this cannot be said of Usher's references.

University of California, Los Angeles

TRUESDELL S. BROWN

LOOKING FOR DILMUN. By *Geoffrey Bibby*. (New York: Alfred A. Knopf. 1969. Pp. ix, 383, vii. \$10.00.)

IN the best British tradition, this is a tale of archeological adventure and discovery in the Middle East. Sir Austen Henry Layard, whose accounts of the rediscovery of Assyria sold like hot cakes a century ago, would have approved of *Looking for Dilmun* and its British-born author, Geoffrey Bibby. Not an archeological report in the formal sense but a trade book, this story of Danish exploits in the region of the Persian Gulf is nevertheless welcome because news of the work has not been easily accessible.

Dilmun, the mythical Sumerian paradise, had long been assumed to have been located on Bahrain Island in the Persian Gulf. At the end of 1953 the Danes commenced to explore for surface finds of flint and pottery and to dig in some of the burial mounds that dot the island by the tens of thousands. From this they progressed to the excavation

of a temple site of the third millennium B.C. and a stratified tell crowned by a Portuguese fort and covering a succession of "cities" going back to a period several hundred years earlier than the (Barbar) temple. A new civilization, called the Dilmun for want of a better term, had been uncovered. Contemporary with Mesopotamia from the Sargonid to the Neo-Babylonian age, the most interesting phase of the Dilmun civilization was that down to the Kassite period when Bahrain had trade relations with not only Mesopotamia but also the Indus Valley. It used the Indus system of weights and stamp seals resembling, but distinguishable from, the Indus seals.

The successes on Bahrain led to invitations to dig elsewhere: in Kuwait, on the Qatar Peninsula, in Oman, and even in Saudi Arabia. These invitations were tied up with the big money going to the Arabian states for their oil, and the Danes discovered that, at least for a while, oil and archeology mixed quite well. Ultimately, however, one gathers that the excavators had so many projects that much of their work was little more than exploratory. They were, as my old Greek professor would have said, in the position of the man who put his foot into more than he could chew.

The Danes had found Dilmun and a new civilization, Ubaid remains on the Arabian coast, and a Hellenistic Greek town on Failaka Island, not to mention many paleolithic sites—quite a bit, but in a tantalizing way not enough. The work has all the earmarks of a good beginning.

University of Minnesota

TOM B. JONES

COINS AND ARCHAEOLOGY. By *Lloyd R. Laing*. (New York: Schocken Books. 1970. Pp. xvi, 336. \$9.50.)

THIS is primarily a book on numismatics for archeologists, set down in a generally historical context. Numismatics has developed so hugely that no single scholar can possibly know the whole field. Archeologists thus sometimes hesitate to discuss even simple numismatic matters. This book will not make numismatists out of them, but it will introduce them to the field.

The book ranges from the first coinage in the seventh century B.C. through Greece and Rome and into the Middle Ages. Coins affecting British archeology are emphasized, especially for the Roman period. Techniques of coin production and the use of punches, dies, molds, and materials are discussed. Typical issues and illustrative problems are brought in.

The reviewer noted a considerable number of errors. A coin of C. Pulcher, cited as a key for dating the *denarii* of the Roman Republic, is on page 26 dated to 98 B.C. and on page 44 to 106 B.C. Both dates are too late. On page 44 Laing says that "the three earliest hoards of republican silver coins are from Masera, from Riccia, and S. Giovanni." There are several earlier silver hoards.

One thing to be learned from this book is that numismatic evidence must be used with great care. In fact, numismatic evidence not supported by any other becomes almost dangerous. Illustrations of these dangers are pointed out by Laing. However, if correctly summarized here, it would seem that other quite unsound research is accepted as valid. For example, it appears that the numbers of coins of a given period discovered in Britain from hoards, stray finds, and formal excavations have been taken to indicate in a relative way the total amount of currency in circulation at that period. But hoards are available to us only by accident of survival—often because of the untimely demise of the original owner. Consequently, more coins may survive

from a troubled age than from a peaceful one, with little relationship to numbers of coins in circulation.

Despite the few inevitable defects, the book will serve its purpose well. It cannot always be trusted in detail, however, and archeologists should read further in their fields. To this end there is a bibliography, which, though full, has inexplicable gaps. The most important book on early Roman coinage, a three-volume work by Rudi Thomsen, is left out, and there are other lapses. The plates are well done and useful, as are the index and the glossary of numismatic terms.

University of North Carolina, Chapel Hill

HENRY C. BOREN

THE OUTBREAK OF THE PELOPONNESIAN WAR. By *Donald Kagan*.
(Ithaca, N. Y.: Cornell University Press. 1969. Pp. xvi, 420. \$10.00.)

THE title of this book requires explanation, since the author deals in great detail with the whole period between the Persian and Peloponnesian Wars (478–431 B.C.). But it is his contention that the development of the Athenian Empire did not inevitably lead to war; rather, the conflict that broke into the open in 431 resulted from the folly of certain minor powers and from miscalculations by the two “superpowers.” The true causes of the Peloponnesian War were those that Thucydides listed as the immediate grievances; he was mistaken in thinking that Athenian imperialism was bound to frighten the Spartans into an inevitable war. Modern historians, too, are in error when they postulate economic and other abstract causes to explain the war. The result of the author’s views is an elaborate diplomatic history of the so-called *pentekontaetia*—from the formation of the Delian League to the outbreak of the Peloponnesian War.

Kagan is optimistic about the feasibility of reconstructing the history of this difficult period. He takes the view that Thucydides wrote his history essentially at one time, that what he tells us is basically accurate, and that the speeches in good part reproduce actual speeches (but he is not sufficiently concerned with the precise meaning of the phrase *tēs xymphasēs gnómēs*, which Thucydides applies to actual speeches in the famous chapter on method, 1.22.1). His major criticism of Thucydides is that he omitted events that did not fit into his preconceived causal scheme. Where Thucydides is not available, the author puts a good deal of trust in Plutarch and Diodorus, although he does not prefer them where Thucydides gives a different account—a method widely used by ancient historians, but one that has always seemed rather peculiar to me. In addition, the author makes good use of inscriptional evidence as presented by the authors of *The Athenian Tribute Lists*, and he discusses a number of problems in brief appendixes. But his main purpose is to give a clear and intelligible narrative: he is not one to avoid decisions on such knotty problems as the chronology of Cimon, the peace of Callias, Pericles’ Pontic expedition, or the trials of Phidias and Anaxagoras. The book serves also as a descriptive history of the period, and the political motives of the leading factions of the day are developed with a clarity that derives from a strong historical imagination and repeated parallels with modern diplomatic history.

The most fruitful idea in the book is perhaps the consistently applied connection between internal factionalism in the Greek cities and their external policies. The author begins with an analysis of Sparta’s external weakness in the leadership of the Peloponnesian League (in particular she could not control Corinth and Thebes), and her internal weakness caused by quarrels between the kings, kings and ephors, and among the ephors themselves. Next he traces the origin of the Delian League

and its early development with due reference to the different policies of Themistocles, Cimon, Ephialtes, and Pericles. The high point of the book's first part is the great crisis of the so-called First Peloponnesian War (about 460–446 B.C.) and the disaster of the Egyptian campaign. It was this war that, in the author's opinion, resulted in the moderate policies of Periclean imperialism in the period of the peace with Sparta (446–432 B.C.). Thus there was no reason for conflict between Athens and Sparta when, about 435 B.C., the affair of Epidamnus and Corcyra began to upset the balance of power.

The second high point of the book is, therefore, the analysis of the events immediately preceding the outbreak of war. The author shows that Corcyra and Corinth acted entirely out of selfish motives, without regard to international consequences. Although Pericles could not allow the Corcyrean fleet to be placed on the side of the Peloponnesian League, Athenian support for Corcyra was so moderate that Pericles' aim was clearly to avoid a major conflict. A similar moderation was shown in the famous Megarian Decree and the ultimatum to the Corinthian colony of Potidaea (but the discussion of the Megarian Decree is rather unconvincing). At the most, Pericles hoped to fight Corinth without the participation of the Peloponnesian League. These were miscalculations, and Athens found herself in a position where she could no longer satisfy Spartan demands. Unwittingly, Pericles had helped the Spartan war party's return to power, and Sparta came to indulge in the illusion of a short and decisive war.

Much could be said about this interpretation. One question that arises concerns the famous Periclean restraint: was it consistent policy or merely a series of temporary expedients to postpone the conflict as long as possible? More important is the question of whether the author has not failed to see the events in the context of fifth-century ideas of war and peace, and empire versus city-state: in a sense, the Athenian type of "hegemony" was unacceptable to contemporary Greece. A third problem is the validity of the comparisons with modern diplomatic history. The book was written when the relations between this country and the Soviet Union were fairly stable, but some of the author's interpretations acquire a different flavor when read under the impact of the renewed Middle East crisis and the president's speeches of November 4, 1969, and April 30, 1970. If the Peloponnesian War was caused by the self-interest of minor powers and the miscalculations of the "superpowers," one can only hope that history does not repeat itself.

It should be noted that there are an excessive number of misprints in the Greek. Otherwise the book is well produced, well written, and gives much food for thought.

University of North Carolina, Chapel Hill

HENRY R. IMMERWAHR

MILITARY THEORY AND PRACTICE IN THE AGE OF XENOPHON.

By J. K. Anderson. (Berkeley and Los Angeles: University of California Press. 1970. Pp. viii, 419. \$12.50.)

In classical Greece only Sparta kept a standing army, and until Leuctra (371 B.C.) it was regarded as invincible. Despite this reputation it is known to us through most of its history only casually; for the sources are civilian and alien and are further hampered by Lacedaemonian state secrecy. Just before the eclipse an expert witness emerged, one who had been successively a seasoned general and a senior Spartan staff officer—Xenophon of Athens. In the book under review his testimony is set forth, collated with the other evidence, and placed in historical context. The author, Professor J. K.

Anderson, has impeccable credentials as a Xenophontean and as an archeologist (see, for example, his *Ancient Greek Horsemanship* reviewed in *AHR*, LXVII [January 1962], 463-64).

Five chapters treat the "organization" of the Spartan army: equipment, commissariat, camps, chain of command, weapon drill, and tactical training. Anderson's most significant discovery is that by 431 the hoplite had discarded body armor and helmet and for defense depended on his shield. The conclusion is firmly based on contemporary representations; a selection is presented in the nineteen plates. Anderson also shows how, once battle was joined, the difficulty of communication made any change in plans virtually impossible.

Five more chapters treat the encounters of 401-362 and demonstrate, first, that the "organization" sometimes affected the outcome; and, second, that the best commanders were tactical theorists more than plain, blunt soldiers. In his outline of the clash at Sardis (395), Anderson, rejecting current orthodoxy, prefers Xenophon to the Oxyrhynchus historian; likewise for Leuctra he accepts Plutarch rather than Diodorus. He makes it seem likely that it was in fact Xenophon who reformed Agesilaus' cavalry in 396-395, and he gives new point to Antalcidas' aphorism that Agesilaus was the teacher of the Thebans.

The biggest novelty in the book is Anderson's treatment of the *Cyropaedia*: he establishes that, whatever its status as "the earliest historical romance," it was evidently intended also as a military handbook. It is used with masterly plausibility to furnish details on such matters as Spartan provisioning and the preliminaries of combat and to illuminate the battles of Nemea and Leuctra.

An appendix tackles the subdivisions of the Spartan army. Xenophon, it concludes, is self-consistent and need not be made to conform with earlier or later writers. The hypothesis is beyond proof, but it has the rare virtue of reasonableness.

My bias constrains me to note one minor slip: on page 115 Anderson has read "bows" for "arrows" in *Anabasis* 3.3.7.

Military Theory and Practice in the Age of Xenophon is a good book, written with authority. It will be indispensable for all students of ancient warfare.

Victoria College, Toronto

W. McLEOD

American School of Classical Studies, Athens

THE ARCHEOLOGY OF THE NEW TESTAMENT: THE LIFE OF JESUS AND THE BEGINNING OF THE EARLY CHURCH. By Jack Finegan. (Princeton: Princeton University Press. 1969. Pp. xxiv, 273. \$20.00.)

THE book under review is much more technical in detail than the previous treatment of this matter by the same author in his book *Light from the Ancient Past* ([2d ed., Princeton, 1959], 297-327). Photographs—296 in all—and plans of the sites mentioned in the Gospels are provided with comments containing ample references to ancient sources and recent scholarly discussion (pp. 1-260). Not only the pictures of landscapes and of the rare monuments reaching back to New Testament times are contained here, but also many buildings connected with later and even apocryphal traditions.

The material is arranged according to the sequence of the Biblical narratives: the life of John the Baptist and the life of Jesus, from Bethlehem and Nazareth, through Samaria, Galilee, Decapolis and Jericho to Jerusalem and Emmaus. Caesarea Maritima is included, because it was the Roman capital of Palestine and also Gerasa, but neither Caesarea Philippi nor Gadara are represented here. The sections on the

tombs and on the sign of the Cross are short monographs of their own right, tracing the development from the early historical periods beyond New Testament times. The book includes a chronological list of ancient sources and indexes. The photographs, many of them taken by the author, are in general good, and those provided by the Matson Photo Service are outstanding.

We may hope that Professor Finegan will prepare a similarly useful volume that will provide archeological documentation for the sites mentioned in the Acts of the Apostles, in the Epistles and in Revelation.

Some marginal remarks: Slight inconsistencies in Hebrew and Arabic words can be easily corrected. The maps may be adapted to the present situation (for example, King Hussein Bridge over Jordan, p. 8). *Meerbal* (Phoenician *mhrb'l*) means rather "soldier" than "gift" of Baal (see *mhr* in Ugaritic, p. 190). The author's proposal of *ostatōn* (p. 237) does not fit into the patterns of Greek formation of nouns.

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STANISLAV SEGERT

Medieval

GARLANDIA: STUDIES IN THE HISTORY OF THE MEDIAEVAL UNIVERSITY. By *Astriq L. Gabriel*. (Notre Dame, Ind: Medieval Institute, University of Notre Dame. 1969. Pp. xv, 287. \$11.90.)

In the light of current campus tensions, there is a pleasurable nostalgia associated with the memory of an earlier age when students and masters together shared the delights of learning in "that Helicon of mediaeval happiness," the Latin quarter of Paris, "in Garlandia." One is indeed grateful that so much has survived of that past that could inspire a contemporary author to exclaim: "Fortunate city in which the students are so many." The present collection of essays demonstrates clearly the wealth of contemporary materials from which Dr. Gabriel has, with affectionate regard for his alma mater, culled information for his subject: "the life of students and masters who were once together in *Garlandia*." The author has given particular attention to the English masters who contributed greatly to the intellectual ferment and achievements of the twelfth century, as well as to the English-German nation in the fifteenth century when the membership comprised Germans, Scandinavians, Hungarians, and other non-English scholars, of whom Martin de Bereck, "Receptor, Proctor and Rector," is an interesting example.

Furthermore, Dr. Gabriel has pointed up the relationship of the cathedral schools of Nôtre Dame to the beginning of the University of Paris and also, possibly as a result of the decline in the following centuries of adequate training for seculars in such schools, the necessity and provision for "preparatory teaching," particularly in grammar and logic, in the Parisian colleges.

Moreover, the author has provided an insight into the life of the students in several of the essays and particularly in that on the "Inconstant Scholar." He has drawn attention as well to the charitable nature of the colleges, not only in his account of the unsuccessful efforts of Johannes Hueven of Arnhem to establish a foundation for the *bursas* or scholarships for three scholars in the College de Sorbonne, but also in the chapter on the "Motivation of the Founders of Mediaeval Colleges."

Aspects of the curriculum—primarily that of the faculty of arts—are cursorily described in the aforementioned chapter on preparatory teaching in the colleges and

also in the essay on "Metaphysics in the Curriculum of Studies of the Mediaeval Universities." Metaphysics, as Dr. Gabriel has noted, constituted one of the subjects of natural philosophy in the three philosophies (rational, natural, and moral) that comprised the spectrum of courses taught in the arts faculty.

A further chapter deals with the heraldry of university seals. Here the author has provided interesting details, together with ample illustrative plates, on the significance of the book in medieval university coats of arms. The book that appeared on medieval university seals, as Dr. Gabriel concludes, has a multiple significance. It "symbolized either the Bible," on which the various university oaths were taken, or the "textbook in the hand" of the master, or it was the "symbol of learning in Lady Wisdom's possession."

Although all of the essays here collected have been previously published, Dr. Gabriel has rendered a signal service to scholars by bringing them together for convenient reference. Their attractiveness and usefulness have been enhanced greatly by some forty illustrative plates, an admirable bibliography of both manuscript and printed materials, and appendixes containing lists of university rectors and of receptors and sub-beadles of the English-German nation, in the fifteenth century. There is also an excellent index.

Hunter College

PEARL KIBRE

PAPST INNOCENZ III. UND DIE KREUZZÜGE. By *Helmut Roscher*. [Forschungen zur Kirchen- und Dogmengeschichte, Number 21.] (Göttingen: Vandenhoeck & Ruprecht. 1969. Pp. 323. DM 39.)

THE thesis of Dr. Roscher's book, which in its original form was a doctoral dissertation in the theological faculty at Göttingen, is that Innocent III changed the character of the Crusades. Dr. Roscher argues that the early Crusades, up to and including the Third Crusade, had been fundamentally under the control of Europe's monarchs and secular magnates. But, according to Dr. Roscher, Innocent III brought the crusading expeditions of the early thirteenth century gradually and inexorably under the immediate control of the papacy. Innocent thus converted the Crusades for the first time into instruments of papal policy. The author further maintains that this shift in direction was confirmed and maintained by Innocent's successors. This shift in the management and control of the Crusades from the hands of the monarchs to the hands of the popes, according to Roscher, underlies the other changes of the crusading enterprise in the thirteenth century. It accounts for the proliferation of Crusades whose geographical focus was in Europe rather than the Holy Land. It accounts for the increasing missionary emphasis in the thirteenth-century crusading enterprises. It also accounts for the increasingly political emphasis of the Crusades during and after the pontificate of Innocent III.

This thesis is intriguing and raises challenging problems of interpretation. There are also some objections to it. For one thing, it is not really so clear as the author seems to believe that the twelfth-century Crusades were controlled by the monarchs. Certainly he discounts too heavily the role and influence of Crusade preachers and legates commissioned by the popes in the earlier expeditions, and he virtually ignores the influence of the popular preachers and other unofficial rabble-rousers. The realities of planning and directing Crusades were not quite so neat and cleanly patterned as Roscher makes them out to be. Moreover, the author intentionally leaves aside the

juridical development of the Crusades and crusading institutions, even though these bear directly upon some of the central questions his argument raises.

Over all, this is an important and ably argued thesis that should stimulate both controversy and further study.

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JAMES A. BRUNDAGE

BEDE'S ECCLESIASTICAL HISTORY OF THE ENGLISH PEOPLE. Edited by *Bertram Colgrave* and *R. A. B. Mynors*. [Oxford Medieval Texts.] (New York: Oxford University Press. 1969. Pp. lxxvi, 618. \$17.75.)

IF Charles Plummer's famous edition of the *Ecclesiastical History* was a milestone in Bedan studies, as the present editors correctly affirm, this work is another. This new edition and translation of Bede embodies the magisterial and elegant scholarship one would expect of Sir Roger Mynors and the late Bertram Colgrave, and a short review can merely celebrate its publication. "The whole edition is intended for the average student, to provide the best possible text, an adequate translation, notes which will explain some of the difficulties met by the modern reader, and guidance as to where to find further information." These modestly stated goals are not only splendidly achieved, but the editors have placed all in their debt—specialist and nonspecialist alike—who turn to "one of the most popular history books in any language."

Mynors, who edited the Latin text and wrote the admirable textual introduction on the manuscript texts in England and on the Continent, has printed the *m*-text (Plummer's "M-type") with the *c*-text ("C-type") variations given in the notes, but has wisely avoided individual manuscript readings of such a reliable text. He also uses the important mid-eighth-century Leningrad manuscript, which was unknown to Plummer. Colgrave's translation, far from merely "adequate," is, in my opinion, the most readable, accurate, and thoroughly satisfactory one in English, true to the spirit and letter of its great original. Colgrave also wrote the notes—less full than Plummer's, but suitable and incorporating recent scholarship—and the historical introduction on Bede's life and times, the models, sources, and style of his *History*, and a sensible answer to "how . . . our greatest medieval historian can spend so much time telling wonder tales." The beautifully printed volume includes Cuthbert's letter on the death of Bede and the Moore Manuscript Continuations, as well as a bibliography and indexes. The late Professor Colgrave, to whose many contributions to our knowledge of Anglo-Saxon England this is a superb capstone, and the distinguished Sir Roger Mynors have done justice to this book, which "became a pattern and gave a new conception of history to western Europe."

Lawrence University

WILLIAM A. CHANEY

REGESTA REGUM ANGLO-NORMANNORUM, 1066–1154. Volume IV, FACSIMILES OF ORIGINAL CHARTERS AND WRITS OF KING STEPHEN, THE EMPRESS MATILDA, AND DUKES GEOFFREY AND HENRY, 1135–1154. Edited by *H. A. Cronne* and *R. H. C. Davis* in continuation of the work of the late *H. W. C. Davis*. (New York: Oxford University Press. 1969. Pp. viii, 31, 50 plates. \$14.00.)

ONCE again, I appear covered with confusion, for when discussing Volume III of the *Regesta Regum Anglo-Normannorum* (*AHR*, Dec. 1968) I, perhaps unjustifiably, assumed that this was the end. Now there has appeared a fourth volume, which is in

the nature of icing on the cake. For the ordinary run of medievalists, the first three volumes will be more than adequate, and only specialists in diplomatic and paleography will be able to profit directly from this admirably produced and—for this day and age—modestly priced volume. On the other hand, every student of twelfth-century England should read the introduction, which is probably the clearest exposition ever written of the techniques that enable specialists to distinguish genuine charters from the inflated, spurious, and forged varieties. It is hard to escape the conclusion that the editors maintained their headquarters somewhere in Baker Street. The amount of scholarly detective work that has gone into these twenty-three pages is obviously enormous, and even the uninitiated cannot fail to be impressed—and amply rewarded—by reading this clear but concisely written essay. Also included in the prefatory material is a list of the last known location of all the extant charters and writs of King Stephen, Queen Matilda, the Empress Matilda, and Dukes Geoffrey and Henry.

The major portion of the volume, however, is given over to the facsimile reproduction, by fine-screen offset process, of the seals of King Stephen (two plates) and of fifty-nine documents, (forty-eight plates) genuine and otherwise, attributed to the chanceries of the above listed magnates. The plates are numbered to conform to the listing in Volume III. To the editors, H. A. Cronne and R. H. C. Davis; to their one-time collaborator, Charles Johnson, and to the initiator of a project so fittingly culminated, H. W. C. Davis, English medievalists will be eternally indebted.

University of North Carolina, Greensboro

JOHN BEELER

THE REIGN OF STEPHEN, 1135–54: ANARCHY IN ENGLAND. By *H. A. Cronne*. [Studies in Medieval History.] (London: Weidenfeld and Nicolson. 1970. Pp. xiii, 313. 70s.)

THE TROUBLED REIGN OF KING STEPHEN. By *John T. Appleby*. (London: G. Bell and Sons; distrib. by Barnes and Noble, New York. 1969. Pp. 218. \$6.75.)

UNTIL the very recent past there had been published but a single full-length study of any aspect of King Stephen's turbulent reign—J. H. Round's oft-cited *Geoffrey de Mandeville* (1892). Now, within the span of four years, three books have appeared that are focused on the events from 1135 to 1154: R. H. C. Davis' *King Stephen* (1967) and the two under consideration here. Professor Cronne and Mr. Appleby approach their subject with widely differing points of view, and their conclusions are irreconcilable.

Professor Cronne's book is not a connected narrative of the reign, but rather a series of nine essays on various social, political, and administrative aspects of the nineteen winters "that Christ and his saints slept." Almost any one of these perceptive pieces could stand alone, but as a result, there is a good deal of repetition that is rather irksome to the reader. Nevertheless, *The Reign of Stephen* is the sole attempt to date—and a highly successful one—to get beneath the murky surface and to piece together, from very scanty evidence, the ways in which the machinery of government was kept running. There has long been a suspicion that, despite the disorders that characterized most of the reign, the complex administrative and judicial systems built up by Henry I and his able subordinates managed to survive, at least in attenuated form. Otherwise there is no way in which to explain satisfactorily the rapidity with which they revived in the early years of Henry II. In the final essays, dealing with the royal household, the Chancery, revenue, law, and the administration of justice, it is

clearly demonstrated that although there was some breakdown in the governmental structure, particularly after Stephen's capture at Lincoln in February 1141, the basic routines were maintained. Toward the beginning of the 1150's there was even a marked increase in the tempo of activity. As far as Stephen is concerned personally, Professor Cronne has no very high opinion of his capabilities. The brains behind Stephen's successful coup in 1135 were those of his younger brother, Bishop Henry of Winchester; the king's real military abilities are denigrated, but it is conceded that he had a certain competence in judicial and administrative business (p. 187). Unfortunately, no attempt is made to pull all these facets together into an ordered whole, and the lack of a concluding chapter may be bothersome to the general reader. Nevertheless, *The Reign of Stephen* is invaluable in filling a long-existing gap in the administrative and judicial history of twelfth-century England.

By way of contrast, John T. Appleby's account of Stephen's reign is an unabashed apologia for that monarch's numerous misfortunes. His weaknesses are glossed over; his strong points are overstressed. Only his lack of Norman sternness prevented his being a successful king. The villains of the piece are the wicked and self-seeking magnates of whom Geoffrey de Mandeville and Earl Ranulf of Chester are only the most conspicuous examples. *The Troubled Reign of King Stephen* is indeed a fast-moving narrative of campaigns, battles, and sieges, based largely on the contemporary chroniclers, whose statements Mr. Appleby is too prone to accept at face value. The use of medieval "statistics" without a caveat as to their general unreliability (pp. 52, 54), and the implication that the stereotyped accounts of devastation and destruction were applicable to the entire kingdom (p. 133) are examples of this tendency. Little attempt has been made to probe beneath the surface, and little that is new will be found in these pages. The general reader, however, will find this book a lively, generally accurate, and useful account. It is a welcome addition to the scanty literature on the reign.

University of North Carolina, Greensboro

JOHN BEELER

THE ORIGINAL STATUTES OF CAMBRIDGE UNIVERSITY: THE TEXT AND ITS HISTORY. By M. B. Hackett. (New York: Cambridge University Press. 1970. Pp. xix, 398. \$16.50.)

At a time when the founding of new universities and the "restructuring" of older ones are highly fashionable, it is refreshing and most instructive to contemplate the early stages in the growth of what now appears to be the first real university constitution, as these are searchingly examined in Dr. Hackett's distinguished book. It is the outcome of a major manuscript discovery: a text of the earliest statutes of Cambridge University, found in 1960 by Dr. Hackett in the final folios of a manuscript he identifies as a thirteenth-century Cambridge grammar teacher's collection come to a somewhat unlikely resting place in the Angelica Library in Rome. Although this manuscript had been noticed some years earlier by a Norwegian scholar, he had failed to exploit his find, and full credit must go to Dr. Hackett for recognizing and demonstrating the significance of his discovery of a document unique among medieval university records. Containing the earliest surviving copy of the first Cambridge statutes, which he ingeniously dates at about 1250, Angelica Manuscript 401 makes it possible to reconstruct the original text and to explore the history of what is far more than a mere collection of statutes, indeed, not only the earliest Cambridge constitution but the earliest extant constitution of any European university.

Why the new university that originated in the dispersion of Oxford masters and scholars in 1209 was the first to give its statutes this coherent form and why its pioneers chose the "distant marsh town" of Cambridge are among the many questions answered or illumined by this wide-ranging yet always sharply focused investigation. For, not content to present simply a critical edition and translation of this important text, Dr. Hackett has made it the heart of a study showing with rare skill and learning just how and what the statutes, closely examined in their varied contexts, contribute to our knowledge of the early history of Cambridge and other universities. Viewed comparatively, this product of Cambridge exigencies and experimentation casts revealing light not only on Oxford institutions, the maternal model that Cambridge by no means slavishly imitated, but on the contemporary university system in general. The greatest significance of the statutes lies, however, in the distinctive picture they give of the actual organization of mid-thirteenth-century Cambridge, possessing already the corporate and academic structure of a *studium generale*.

Though they reveal a good deal about university government vested in the chancellor and the regent masters, his electors, about academic procedure and discipline, and about lodgings and schools, the statutes are virtually mute about courses and studies. For these we must look to later recensions and particularly the new material supplied in Dr. Hackett's astute analysis of them, which shows that in this case, as in many others, the early text provides an essential measure of change, a base from which to trace the evolution of the Cambridge constitution, as he has done, to the end of the Middle Ages. If his discovery finally places the medieval history of Cambridge on a firm foundation, his study makes an indispensable contribution to many aspects of this history and gives the university a larger importance than historians have hitherto recognized.

Millbrook, New York

MARY MARTIN McLAUGHLIN

A HISTORY OF BUBONIC PLAGUE IN THE BRITISH ISLES. By J. F. D. Shrewsbury. (New York: Cambridge University Press. 1970. Pp. xi, 661. \$25.00.)

In the good old days disease hung like the sword of Damocles over the heads of men. Referring to smallpox, "Lord" George Sanger observed early in this century: "People in these days cannot imagine what the scourge was like, what a thrill of fear and horror it produced. . . . In some places, notably in seaport towns, the slums of London, and other large cities, it lurked regularly, and people were, in a sense, accustomed to its presence. But now and again . . . it burst forth into a tremendous pestilence that stalked the length and breadth of the land." This characterization can just as easily be applied to plague.

Two great epidemics have been considered as marking the onset and the waning of the medieval world, the plague of Justinian (543 A.D.) and the Black Death (1348-49). Between these two dates and for several centuries thereafter, Europe and the European littoral were visited and ravaged by larger or smaller outbreaks of disease, among which plague epidemics played an important part.

Professor Shrewsbury studies the epidemic history of Britain from the middle of the fourteenth century to the middle of the seventeenth century, focusing on outbreaks of bubonic plague. The book deals with two problems: what did "plague" mean to those who used the term during the period studied? And how effective was the plague as a killer—in short, how many people died? Shrewsbury takes off from the basic biology of plague—black rat, rat flea, and man—and uses this ecological

triad to distinguish plague from other diseases and to establish their roles during plague periods. For example, it is not generally recognized that a vector may transmit two different diseases—the louse transmits typhus fever and relapsing fever—and in periods when the disease could not be distinguished bacteriologically there was a great deal of clinical confusion. This is what Professor Shrewsbury does for bubonic plague in Britain.

This is an excellent scholarly work and must be considered by everyone interested in social history, historical demography, and certainly those concerned with the history of disease, medicine, and public health.

Yale University School of Medicine

GEORGE ROSEN, M.D.

THE GOTHs IN SPAIN. By E. A. Thompson. (New York: Oxford University Press. 1969. Pp. xiv, 358. \$9.95.)

WORKING forward from his shorter books on the *Early Germans* (1965) and the *Visigoths in the Time of Ulfilá* (1966), Thompson takes up sixth- and seventh-century Spain, where intruding Visigoths presided over the surprisingly healthy remnants of Roman civilization. He does not attempt a general history like that of Torres-López and his colleagues in volume III of the Menéndez-Pidal *Historia de España* (1963), nor does he try to cope with the flow of articles currently testifying to renewed scholarly interest in all aspects of that era. Instead he probes the governmental functioning of the Gothic ruling minority, especially in its interplay with the subject Romans, including the evolving relations with the Church. Despite its deliberately narrow focus, the resultant combination of synthetic overview and topical inquiry amounts to the best political history of Visigothic Spain in English. Proceeding chronologically by kings (he devotes some 150 pages to the Arian kingdom and 200 to the Catholic and includes an appendix on the Byzantine province), Thompson argues for an odd dual state—separate but equal administrations, nonexploitative—until the mysterious crises of about 600 and 650 caused the Gothic element to prevail at the legal-administrative level, while paradoxically hastening the sociocultural Romanization signaled by Reccared's conversion.

With surviving documentation so exiguous, the book inevitably treads ground familiar in its general features; it brings freshness and conviction, however, by highlighting the important questions, raising a number of its own, providing forthright and knowledgeable interpretations where seemly, and by honestly facing up to our inability as yet to solve some vital problems. The author's choice of interpretations of course is bound to encounter opposition, but at least he exposes the range of questions to be answered. *Goths* rests on meticulous citation of sources, while incorporating the best recent scholarship, such as the articles by Hillgarth and Murphy, as well as standard older works like the monographs of the Ziegler school. Lacunae do appear, however, as when the author discusses at length Goths and Romans in the episcopacy without reference to Orlandis' recent article, and when he confesses that Bishko's "other works" were "inaccessible" to him (p. 292n.). He provides a good index but no bibliography or maps.

Though the book's scholarship makes it a welcome addition to the growing bibliography on the Visigoths, especially to the meager English section, its lucid order and direct vigorous style recommend it also for undergraduate courses in early medieval civilization and in Spanish history.

University of San Francisco

ROBERT IGNATIUS BURNS, S.J.

GESCHICHTE DER FRANKEN BIS ZUR MITTE DES SECHSTEN JAHRHUNDERTS. By *Erich Zöllner*. Based on the work of *Ludwig Schmidt*, with the collaboration of *Joachim Werner*. [Geschichte der deutschen Stämme bis zum Ausgang der Völkerwanderung.] (Munich: Verlag C. H. Beck. 1970. Pp. viii, 278. DM 45.

THIS handbook on the early history of the Franks supersedes that by Schmidt published in 1918. Zöllner is well aware of the thanklessness of his task, as even so complete a revision as his still requires impersonal presentation and adherence to his predecessor's outline. Thus Zöllner, while fully realizing the importance of his subject (or as he put it, "no other tribes participating in the Barbaric Invasions had more lasting political success than the Franks and the Anglo-Saxons"), explains neither the peculiar dynamics of Frankish expansion nor the structural bases underlying Frankish power. By following Schmidt's division of the presentation into a straight historical narrative and a topical analysis of the various facets of Frankish life, his approach not only becomes fragmented, but also limits him to describing primarily Salic institutions. One is reminded somewhat of the charges made by German historiography against ancient authors, especially Tacitus, to the effect that their schematic approach tended to reduce their ethnographic accounts to exercises in the presentation of *a priori* concepts (*Topoi*).

This criticism should in no way diminish the excellence of the contribution made by the author, who has re-examined all contemporary accounts, related them to the results of historical and archeological scholarship of the last fifty years, and provided the reader with the benefit of his solid professional judgment in clarifying disputed points of fact and theory. A bibliography of contemporary authors, a genealogical table, and two original maps round out this well-indexed work.

One hopes, as the author certainly would, that his effort will form the basis for more insights into the Frankish phenomenon.

Library of Congress

ARNOLD H. PRICE

SAVONAROLA E LA CURIA ROMANA. By *Romeo De Maio*. [Uomini e dottrine, Number 15.] (Rome: Edizioni di Storia e Letteratura. 1969. Pp. 259.)

IN view of the fact that writings about Savonarola have been predominantly polemical, it is refreshing to note that this monograph, dealing with the relations between the stubborn friar and the Roman Curia and written by a disciple of the late Delio Cantimori, has achieved a very high degree of objectivity. These relations in effect revolved around the attitudes and policies of Oliviero Carafa, cardinal of Naples, protector of the Dominican Order, and probably the most influential prelate at Alexander VI's court. Despite the accumulation of many benefices, Carafa was a leading advocate of Church reform in terms of monastic discipline and eradication of abuses with due respect to hierarchic and practical considerations. This explains the cardinal's early sympathy with Savonarola's program, a sympathy that at times was strained by the latter's mystical radicalism, anti-humanist tendencies, uncompromising attitude, and pro-French stand. These differences in approach and personality, however, did not deter Carafa from aiding and protecting the impatient reformer until the very end, even to the point of endangering his own interests and personal safety. Certainly Carafa's role in securing the independence of the monastery of San Marco from Lombard jurisdiction and in moderating the punitive intentions of the pope,

angered by the friar's outbursts against the papacy, can only be understood in the light of the cardinal's influence at the Curia and Alexander's fear of it. Ironically Carafa's well-intentioned fusion of the Tuscan and Roman Dominican monasteries, designed to enlarge Savonarola's reforming influence, was misinterpreted by the friar and led him to obstinate disobedience, which in turn offended the cardinal and supplied the Curia with the juridical pretext for the excommunication. The principal contribution of this book consists in revealing for the first time the full scope of Carafa's role in the drama of Savonarola. De Maio, author of a monograph on another Carafa cardinal, Alfonso, has with this publication made another important contribution to the history of this influential Neapolitan family, which included Gianpietro Carafa, an eyewitness to these events and who later as Pope Paul IV was to denounce the reformer as the Luther of Italy.

The documentation is extensive and includes documents drawn from several archives and libraries in and outside Italy, some of which are newly discovered and appear in the appendixes. Although the book is supplied with well-executed indexes of authors, manuscripts, proper names, and illustrations, it lacks a bibliography.

University of Massachusetts, Amherst

VINCENT ILARDI

Modern Europe

LUTHER: AN INTRODUCTION TO HIS THOUGHT. By *Gerhard Ebeling*.

Translated by *R. A. Wilson*. (Philadelphia: Fortress Press. 1970. Pp. 287. \$5.95.)

LUTHER, ERASMUS AND THE REFORMATION: A CATHOLIC-PROTESTANT REAPPRAISAL. Edited by *John C. Olin et al.* (New York: Fordham University Press. 1969. Pp. x, 150. \$6.00.)

LUTHER: LEBEN—SCHRIFTEN—DENKEN. By *Peter Kawerau*. [Lehrbuch der Kirchengeschichte.] (Marburg: N. G. Elwert Verlag. 1969. Pp. 179. Cloth DM 24, paper DM 18.)

DIE LUTHERSACHE (CAUSA LUTHERI), 1517–1524: DIE ANFÄNGE DER REFORMATION ALS FRAGE VON POLITIK UND RECHT. By *Wilhelm Borth*. [Historische Studien, Number 414.] (Lübeck: Matthiesen Verlag. 1970. Pp. 182. DM 26.)

WHY should a historian read a book about Martin Luther when Luther's own works are so clear and handy? Take Gerhard Ebeling's *Luther*. Ebeling tells us that he wants to get at the "inner dynamic" of Luther's thought "to make clear its contemporary validity." But if Luther has an "inner dynamic," Ebeling certainly has none. His book plods from page to weary page in a desert of lifeless verbiage, and I cannot understand why anyone should read Ebeling when he can so easily have the stinging prose of Luther himself.

But in his own obtuse way, Ebeling raises some issues for historians. Can Luther's history or any other history truly serve the present as sentinel and guide? I rather think it is better for historians to use the present to illuminate the past instead of the reverse. Ebeling admires Luther for standing four-square on the Bible and comments learnedly on Luther's great distinction between law and gospel in the Scriptures. Ebeling says we must know the Bible if we would know Luther. But Ebeling himself ignores the centuries of critical scholarship since Luther that have shown the Bible to be not one grand unity to be judged in terms of law and gospel but rather a con-

tradictory hodge-podge of great literature, sublime opacity, bloodcurdling superstition, and trash. Do we learn from Luther to be utterly uncompromising in the irrational beliefs for which we will live and die and kill others to sustain? One might imagine that the present could have moved Ebeling to ask some telling questions of Luther. For our calamities, like those of the Reformation, are caused not by the excess of skepticism but by an excess of belief. But even as he judges the past, Ebeling shows no sense of the present that makes such judgments worthwhile, and so he does not show Luther to have any "contemporary validity" at all.

In his essay in *Luther, Erasmus and the Reformation* Roland Bainton sees the problems of Luther's claim to perfect knowledge of the Scriptures. He is sympathetic to Luther, but his heart lies with Erasmus who did not think he knew enough to set the world ablaze. Poor Erasmus! In a beautifully poignant essay in this same work, Margaret Mann Phillips shows Erasmus grown old, "sitting at Freiburg . . . like a snail in its shell, with the recriminations of both sides washing up to his door." Mrs. Phillips is moved to ask if Erasmus did not live too long. But were it not for Erasmus, for Rabelais, and for Montaigne, the fanatical "true believers" of their age might have swept civilization away.

Most of the other essays in this book are twaddle. The work grew out of a two-day Catholic-Protestant seance in 1967 on the Reformation. The scholars of the several sects were so thrilled that they could discuss the Reformation without burning each other that they did not notice that nothing much got said. The meeting proved that the only solid accomplishment of the ecumenical movement has been to corrupt scholarship, for in these circles scholarship stands not for itself but as a handmaiden to saccharine religiosity.

Strange voices are raised. Wilhelm Pauck praises Joseph Lortz as the decisive force in Catholic ecumenical feeling. Lortz, whose *Reformation in Deutschland* appeared in 1939, amazed Protestants by praising Luther as representing the best in German piety. Pauck fails to mention that Lortz despised Erasmus. Pauck is also too polite to recall that Lortz was a notorious Nazi sympathizer to the bitter end. I rather think that to Lortz Erasmus represented the cosmopolitanism the Nazi hated. But Luther was a folk hero. Of course Lortz had to praise him. And if now we must build a scholarly foundation for ecumenicism by praising Joseph Lortz, then the proposed edifice is certainly too flimsy to stand.

Peter Kawerau's book is truly fine. He surveys Luther scholarship on various points in the reformer's life to about 1521. Theodosius Harnack, Reinhold Seeberg, Karl Holl, and many others receive judicious consideration as well.

Kawerau suggests that Luther might have imbibed some of the Kabbala speculation of Johannes Reuchlin. This would help to explain both Luther's misguided devotion to the Old Testament and his fervent emphasis on the name of God. The Kabbala was everywhere studied in the Renaissance, and its dark mysticism might well have appealed to Luther's haunted and superstitious mind. Yet I doubt that its influence would have been pervasive. Luther probably never had time to read deeply of anything but the Bible. His knowledge of other things always seems impressionistic and haphazard. He probably knew no more of the Kabbala than he did of Thomas Aquinas.

Kawerau also further erodes belief in Luther's story of his early development with its *Turmerlebnis*. In 1545 Luther wrote down the stormy account of his discovery of Romans 1:17 and justification by faith alone. Most later biographers parroted the story. So we have the heroic story of an agonized young Luther coming to his faith

before the indulgence controversy of 1517. The trouble is that no indisputable evidence of such a discovery appears in any of Luther's many works available from the time.

Kawerau thinks that Luther had only begun his theological development by 1517, that his first great step was to attack the papacy, that as the fight grew more fierce he looked around for any tool he could use and gradually came up with *sola scriptura* and justification by faith. Young man Luther may have had his agonies of soul, but they were probably eased by the pleasures of a good fight rather than by sitting alone with his Bible in a monkish tower.

Wilhelm Borth shuns theology and gives us a splendid account of the political maneuvering that carried the Luther case along from 1517 to 1524. He shows us once again the enormous political advantages Luther possessed. Like Ranke and Holborn, Borth shows that the princes of Germany captured the Reformation almost at the beginning. Whatever may have been Luther's desire, this fact became his ultimate significance in history.

University of Tennessee

RICHARD MARIUS

ROMA SANCTA (1581). By Gregory Martin. Now first edited from the manuscript by George Bruner Parks. (Rome: Edizioni di Storia e Letteratura; distrib. by Parker and Son, Oxford. 1969. Pp. xxxii, 316. \$18.00.)

GREGORY Martin (1542?-1582) was an English Catholic priest who lived on the Continent from 1569 or 1570, teaching in the English college at Douai and later at Reims, after the college had been forced to move to that city. It was he who made the Reims-Douai translation of the Bible. *Roma Sancta*, here printed for the first time, is a description of the piety and devotion of Rome, based partly on the author's personal observations during an eighteen-month sojourn there in the years 1576-78. It is divided into two books, the first on the devotion of Rome, the second on the charity of Rome. He contends that the religious zeal of Rome in his day is equal to that of the Rome of the Fathers. His description of the devotion of the Romans to pious exercises gives a vivid picture of the spirit of the Catholic revival of the period and the forms in which it expressed itself. There is a valuable account of the houses of the various religious orders, including the number of inmates in each. The longest chapter of the book is about the Jesuits, whom he greatly admired. He also describes the hospitals of the city and the various lay confraternities, with emphasis on the work of these confraternities during the jubilee year of 1575.

Martin's own deep devotion to his faith breathes through the entire book. He rejoices in the vast numbers of people who frequent the churches, in the relics, the processions, the practices of the Flagellants who whipped themselves bloody, and in the saintly lives of such men as his friend Edmund Campion and, above all, Cardinal Borromeo, who outshines "al the other lightes that shine in the Church at this day" (p. 253). He praises the virtues of the religious life and is lost in admiration for the hospitality of the confraternities, who welcomed and cared for the jubilee pilgrims of 1575. He glories in the vast concourse of pilgrims to the Holy City in this jubilee, to which he devotes a great deal of attention.

He was implacably opposed to "Luthers carnall and dissolute heresie" (p. 119) and the pernicious doctrine of justification by faith. He deplores England's "long captivitie" (p. 114) and addresses his book to his fellow Englishmen.

The book is very well edited by Professor Parks, though the annotation might have been fuller. Many persons are mentioned in the text who are identified not at all

or only very briefly, and there are some obscure words that are not explained. At the end (pp. 260-62) Martin seems to be refuting some heretical author or authors for whom the editor provides no identification. On the whole, however, the book is a rich source of information on the state of religion in Rome in the late sixteenth century and will be welcomed by all students of the Counter-Reformation.

University of Kansas

WILLIAM GILBERT

THE FIFTY DAYS: NAPOLEON IN ENGLAND. By *Jean Duhamel*. Translated by *R. A. Hall*. (Coral Gables, Fla.: University of Miami Press. 1970. Pp. 141. \$5.95.)

THIS interesting volume, limited primarily to the fifty days Napoleon spent on H.M.S. *Bellerophon* in 1815, presents a detailed account of the emperor's hopes, activities, and treatment while waiting for the allies to determine his fate. Moreover, it describes his decision to take refuge on an English warship and, for the first time, analyzes the legal ramifications and consequences for the Liverpool cabinet. The author traces the emperor's flight from Malmaison to Rochfort, his attempt to secure safe passage, his sickness and vacillation, and the continual pressure from the Paris government to force him to leave French soil, but the author's major contribution is his discussion of Napoleon's legal position. Was he a sovereign, a prisoner of war, an alien, a Frenchman, or an Elban?

Once the *Bellerophon* was anchored off Plymouth, Napoleon and his entourage enlisted the support of sympathetic English with surprising results, even gaining support of the prince regent's brother. The heroic efforts of the barrister Capell Lofft to have Napoleon landed on English soil under the Habeas Corpus Act and the bizarre attempts of Anthony McKenrot to secure Napoleon's person with a writ of *habeas corpus ad subjiciendum* are vividly described by Duhamel as well as the efforts of the cabinet and the admiralty to circumvent them. In fact, the government was so concerned about the legality of their treatment of Napoleon that Liverpool insisted on an indemnity act by Parliament to legalize their actions.

Although he uses important new manuscript sources from the English archives, it is unfortunate that the author did not include footnotes or indicate the specific locations of his archival material. Duhamel has made a good case for vindicating Captain Maitland, but his treatment of Fouché after Waterloo, without noting Davout's belligerent attitude and Becker's opportunism, is less convincing. Nevertheless, this is a well-balanced study, sympathetic to Napoleon and yet sensitive to the problems faced by the British government. This volume, with its smooth and effective translation, is recommended for anyone interested in Napoleon.

Florida State University

DONALD D. HORWARD

THE TEACHING OF CHARLES FOURIER. By *Nicholas V. Riasanovsky*. (Berkeley and Los Angeles: University of California Press. 1969. Pp. xii, 256. \$6.50.)

THIS admirable study, the first of its kind in English, examines the thought of the strangest and in many ways the most remarkable of the Utopian socialists. The author's purpose is to "state Fourier's system in its own terms," to demonstrate that it was "all of a piece," and that his universal formula was "essentially mad."

Riasanovsky successfully accomplishes the first and most important of these aims.

His first chapter reviews Fourier's life and circumstances, noting some of the ways in which they might have influenced his thought. The core of the book, also the longest chapter, is a critical assessment of the phalanx in which Fourier's characterology and his plan for social organization are fully presented. The same chapter includes a discussion of the Harmonian's bizarre cosmology and his religious outlook. The final chapters evaluate his piercing social criticism, the nature of his socialism, and the psychological significance of Fourier's work.

The heart of Fourier's teaching is the theory of passionnal attractions. The twelve fundamental passions and the 810 "principal characters" created by their interplay in individual personalities provide the basis for the mathematically derived complex of human interaction that Fourier called the phalanx. Only in such a unit comprising 1620 to 1800 individuals (the number needed to bring all major character types into relation with one another) could the full liberation of the passions, and therefore true happiness, be possible. And only in such a unit would this liberation create an orderly, efficient, and productive society. Riasanovsky does a better job of explaining the inherent logic of Fourier's social imperative than any other writer I have read. He also makes perfectly clear his Newtonian assumptions concerning the rationality of God's plan and his optimism about man's ability to overcome evil (which, of course, is present civilization).

There are certain aspects of this book that deserve critical comment, however. Riasanovsky asserts that Fourier's thought was thoroughly integrated, that no part of it could stand alone without the rest. It is upon this assertion that he builds the claim of the "madness" of Fourier's teaching. Thus the concept of the phalanx must be linked to cosmic transformation and to such peculiar religious concepts as purely human metempsychosis. I find no compunction whatsoever to challenge Fourier's profound psychological insights, however, just because he thought that new "aromas" emitted by the earth would revitalize the sun and therefore change the climates and the nature of earthly animal life. Fourier's cosmology, which included such notions as self-copulating celestial bodies, may indeed have been thoroughly mad, but the author fails to demonstrate that his theory of social organization was in any way dependent upon it. Insanity here, brilliant analysis there: this is the traditional view of Fourier, and I can see no reason for abandoning it.

The general purpose of the book is also somewhat in doubt. It does not intend to relate Fourier to his age (Frank Manuel has done this well) nor does it wish to gauge his influence; rather the author wants "to place Fourier on the intellectual map of the modern world." It is well known that Fourier was ahead of his times in many respects, that he was perhaps the perfect Mannheim "Utopian." His psychology is admired by many neo-Freudians. It is somewhat distressing, therefore, to witness Riasanovsky's excessive moral neutrality with regard to Fourier's significance for the value crisis of the contemporary West. He refers to the clear link between Fourier and Norman O. Brown but is unwilling to offer a judgment himself as to the contemporaneity of this lonely and troubled son of the French provincial bourgeoisie. The alienation of labor, the vapidness of much intellectual endeavor, the tragedy of the narrow lives of petty bureaucrats and men of commerce, the dangers of over-urbanization, the viciousness of cultural and racial insularity, the repressed lives of women and children under a patriarchal inheritance, the profound anxieties produced by a puritanical sexual ethic, the doctrine of delayed gratification and its relation to the capitalist mentality, the incredible contradiction of poverty in the midst of plenty, and the solution—the liberation of the passions in a naturally ordered

framework—all these Fourierist themes are touched upon and sometimes orchestrated by Riasanovsky. But only by inference may we set them in the contemporary context. Subtlety is fine; it is scholarly. Yet I am saddened that the author first taints Fourier's social thought with "madness" and then avoids the responsibility of really evaluating the current significance of his teaching.

Such reservations aside, it is still impossible to avoid the conclusion that Riasanovsky's work is a clearheaded, well-constructed, and most relevant book.

Wayne State University

CHRISTOPHER H. JOHNSON

THE PHILOSOPHY OF LUDWIG FEUERBACH. By *Eugene Kamenka*. (New York: Praeger Publishers. 1970. Pp. xv, 190. \$7.00.)

THIS compact study is a welcome addition to the limited bibliography in English on Ludwig Feuerbach and a timely companion piece to the recent translations of three of Feuerbach's works published separately by Melvin Chernov, Ralph Manheim, and Manfred Vogel. As the author of several works on Marxist and Soviet thought, Eugene Kamenka was aware of a gap in the existing literature and has sought to fill it in part with a presentation of Feuerbach's ideas that is intended to balance "exposition and criticism." The expository purpose clearly dominates, however, as Kamenka systematically covers Feuerbach's critique of religion and philosophy, his method, and his theories of man, ethics, knowledge, and materialism. The exposition is clear and readable, and he seldom employs a technical vocabulary. On the critical level, the author justifiably emphasizes that as a thinker Feuerbach lacked system, precision, and logical consistency. At various points he interjects other specific criticisms, but taken together they do not add up to the critical essay on Feuerbach's philosophy. Unfortunately, some of the author's interesting comments are in the notes, which are inappropriately relegated to the back of the book.

Historians will be disappointed with Kamenka's admittedly restricted efforts to fix Feuerbach's place in the context of modern intellectual history. The first chapter on the cultural background is fragmentary and not carefully joined to the body of the book. On the other side of the historical problem, that is, on the nature and extent of Feuerbach's influence, there is a comparable sketchiness. To be sure, on the importance of Feuerbach for the early intellectual development of Marx and Engels, a topic analyzed in numerous studies, Kamenka spells out many of the particulars. But he chooses not to elaborate on his own assertions concerning Feuerbach's substantive influence on Søren Kierkegaard, Friedrich Nietzsche, Max Scheler, Karl Löwith, Nikolai Berdyaev, Martin Buber, Louis Althusser, and Jean-Paul Sartre. Once these intellectual connections have been so clearly identified a student of modern thought is understandably disappointed to learn on the next page that the author will offer no further explanation and disclaims responsibility for evaluating Feuerbach's historical influence. Disappointment in this matter, however, does not diminish the usefulness of Kamenka's book as an admirably concise exposition of Feuerbach's ideas.

The Johns Hopkins University

VERNON L. LIDTKE

ENTENTE CORDIALE: THE ORIGINS AND NEGOTIATIONS OF THE ANGLO-FRENCH AGREEMENTS OF 8 APRIL 1904. By *P. J. V. Rolo*. ([New York: St. Martin's Press. 1969. Pp. 300. \$13.50.]

THE author of this study is senior lecturer in history and international relations at Keele University in England. He has given us a commendable survey of the origins and developments of the Anglo-French agreements of April 8, 1904. The purpose of the study, according to the author, is "to trace the origins of those agreements, to examine the negotiations in their contemporary setting and to investigate what wider considerations . . . lay behind the bargains then concluded." The principal sources are the published British and French diplomatic documents and the special studies of others who have used the now available manuscript collections of public and private papers. These include Robinson and Gallagher, *Africa and the Victorians*; Sanderson, *England, Europe and the Upper Nile*; Grenville, *Lord Salisbury and Foreign Policy*; Andrew, *Théophile Delcassé and the Making of the Entente Cordiale*; and Monger, *The End of Isolation*. The first five chapters treat the background and origins of the issues that had a long history of diplomatic bickering and abortive negotiations and that were finally settled in the 1904 accords. Single chapters introduce the principal figures in the making of the accords—Cambon, Delcassé, Lansdowne, and Edward VII. Five chapters deal with the negotiations, which were conducted in London between Lansdowne and Cambon. Each procedural move, each memorandum exchanged, each meeting of the two negotiators is examined in detail. This is tedious reading as the negotiations extended over a period of nine months. Lansdowne and Cambon "bargained furiously, but quietly," in the spirit and manner of the old diplomacy, which would be impossible today when diplomatic exchanges are about as quiet as a brass band. Egypt and Morocco were principal objects of the negotiations—the Newfoundland fisheries, Madagascar, Siam, and African boundary problems were secondary and fell into place when the two most important issues were settled.

In a concluding chapter the author clarifies his position with regard to the significance of the agreement. An alliance, he concludes, was not an inevitable outcome of the Entente Cordiale. Whatever security considerations may have been in Delcassé's mind, Lansdowne never desired a diplomatic revolution. The agreement did reduce previous sources of friction, opened the way for later agreement with Russia, and became what had not been envisaged—an anti-German combination. But it was not the Entente Cordiale per se but rather German military planning—the Schlieffen plan and the threat of the German navy—that brought France, Germany, and Britain to war in 1914.

University of Virginia

ORON J. HALE

THE POLITICS OF GRAND STRATEGY: BRITAIN AND FRANCE PREPARE FOR WAR, 1904-1914. By *Samuel R. Williamson, Jr.* (Cambridge, Mass.: Harvard University Press. 1969. Pp. x, 409. \$12.50.)

PROFESSOR Williamson traces the developing military and naval relationship between Britain and France during the decade following the conclusion of the 1904 Entente and preceding the outbreak of the First World War. It is a story that has been told before, but never in anything like the detail Williamson has been able to provide, owing to his access to extensive archival and private sources. More important, it has

never been told in order to answer the questions Williamson sets for himself: "What effect did the entente and the staff talks have upon Britain's decision to enter the war? And what role did the conversations play in London's adoption of a continental strategy?" (pp. 343-44). In other words, to what extent did the growing web of relationships between civilian officials and military and naval officers in London and their counterparts in Paris alter the range of options open to the British cabinet regarding whether or not to go to war, and, once this decision had been made, to what extent did these relationships commit Britain to a strategy that was, to say the least, not to her comparative advantage?

Williamson concludes that the bureaucratic relationships were important, but not critically important, in affecting the British cabinet's decisions in August 1914, but that they did indeed decisively prejudice the manner in which Britain fought the war. Stated as baldly as this, such conclusions are scarcely surprising. But in reaching them Williamson provides a graphic demonstration of how, in his words, "faceless groups often play great roles," and in doing so he makes a valuable contribution to the growing literature on the bureaucratic politics of international relations. In great detail he examines personal and organizational rivalries within the British and French armies and navies, and between the two services of each country, showing how, in many instances, groups within each service used linkages with opposite numbers in the other country to further domestic parochial ambitions. Superimposed on these two sets of relationships, of course, were those within each society between bureaucrats and politicians. Williamson skillfully examines them as well.

His treatment of the British side is better than that of the French, but his interest clearly lay there: the British, after all, had the widest range of options, and the process by which these options became identified as such and were chosen is Williamson's chief concern. This is not to say that his treatment of the French side is without interest. He has, indeed, been able to exploit with profit the French military archives, which were opened for a short time around 1962, only to be closed again and reopened in 1969.

Sometimes the questions on which Williamson seeks to focus become lost in his maze of detail. One gets the impression that these questions emerged in their full clarity after Williamson had completed his study, and that he was not ruthless enough in rewriting to give them the central focus that he claims for them in his conclusions. Also, he devotes perhaps too much space to the general political relations between Britain and France, and therefore recounts a narrative history that has been told before. His book is, after all, aimed at two sorts of audiences: specialists in the period, and students of bureaucratic politics. The same pruning would have benefited both and made even more powerful what is, withal, a persuasive and well-stated case.

Princeton University

RICHARD H. ULLMAN

THE END OF GLORY: AN INTERPRETATION OF THE ORIGINS OF
WORLD WAR II. By *Laurence Lafore*. [Critical Periods of History.] (Philadelphia: J. B. Lippincott Company. 1970. Pp. vii, 280. \$5.95.)

AFTER writing a thoughtful little book on the outbreak of World War I called *The Long Fuse*, Professor Lafore has now turned to the second great war of this century. In *The End of Glory* he analyzes the impact of World War I on Europe and the world, discusses the major developments of the 1920's, and gives his summary of the background and meaning of the road to war in the 1930's. The first of these three topics

is handled well, the second fairly, and the third so poorly as to make both author and publisher look ridiculous.

The way in which the world was transformed by World War I is and will long remain a significant theme, and Lafore's incisive comments on this subject are welcome indeed. As he leaves the more familiar ground of that era, however, the reader wonders whether haste and carelessness have not been allowed priority over reflection and accuracy. The instances where fact has been sacrificed to felicity of expression are too numerous to recount, but a few examples may illustrate the point. We are told that the violent attacks on the leaders of the Weimar Republic took place after the Rhur occupation—but Matthias Erzberger and Walther Rathenau lay slain before that event. We are assured that Alsace-Lorraine has no Frenchmen and the Polish Corridor no Poles—no wonder Lafore's consideration of the minorities issue is a little strange. But there are more fundamental problems. Not understanding the relationship of the demilitarized Rhineland to East and Southeast Europe, Lafore has no understanding of the crisis of March 1936; unaware of any of Hitler's comments on his aims between January 1933 and November 1937 (such as his memorandum on the Four-Year Plan), Lafore cannot give any meaningful review of Hitler's foreign policy in those years; unfamiliar with either the British government's consideration in the winters of 1935–36 and 1937–38 of colonial concessions to Germany or its new views of German threats in the winter of 1938–39, the author cannot present a picture of British policy that makes any sense. Helped by a few good secondary sources and misled by a few poor ones, the author has gone into print too soon. His original mind, thoughtful analysis, and well-paced style will make his mature reflections on World War II very much worth reading. The sloppy first draft is not. And whether in an age of increasing unity in Europe, divergence between the developed and undeveloped countries, and nuclear stalemate between the superpowers, Europe's role is as finished as the author would have us think may also be worthy of further thought.

University of Michigan, Ann Arbor

GERHARD L. WEINBERG

THE ORDEAL OF TOTAL WAR, 1939–1945. By *Gordon Wright*. [The Rise of Modern Europe: A Survey of European History in Its Political, Economic, and Cultural Aspects from the End of the Middle Ages to the Present.] (New York: Harper and Row. 1968. Pp. xv, 315. \$7.95.)

SOME time ago a brief item in the *Times Literary Supplement* noted this work (together with another volume in the same series). The total comment was that it covered subjects normally treated separately. Perhaps only the anonymity provided by that sometimes capricious journal would provide the courage for so foolish a jibe (though, like many a witless remark, it contained an unintended truth). Had the writer extended himself to open the book, he could hardly have cared to launch such a line, even unattributed. A short study of the Second World War is not really an innovation. The major purpose of the Langer series, as even English undergraduates know, has been to bring together the findings of a wide range of relevant monographs. Mr. Wright's book does this.

It is the twentieth, and final, scheduled volume of the series. With one exception, the period it covers is the shortest—"much too short to reveal the evolution of institutions and ideas," the author remarks, "too short even to justify some sort of descriptive label for the era." Since his purpose was to consider the meaning of this war for

the development of Europe rather than to recount the war's events, he suggests that the six years might better have been the epilogue to the preceding volume (not yet published) or the prologue to some hypothetical volume for the post-1945 scene. Not every reader will be convinced. In the first place, the six years were peculiarly replete with mere recorded events, the principal clusters of which no general history of the time can yet ignore. And secondly, attaching these years either to the 1919-39 book or to some postwar book would diminish the effect achieved here of presenting the war as a colossal happening, accelerating and masking one more of the many eras of transition. Undeniably the present volume presents European history at a level of particularity quite different from the levels found in the contributions of, say, Cheyney, Friedrich, Dorn, or Hayes in the series. But this is a familiar situation, consequent upon the intimidating wealth and even availability of recent materials, problems of perspective, and the inevitable contemporary preoccupations against which general historians of civilization have seldom prevailed.

Less than one third of the text is narrative of the principal political and military events; even these pages are marked by a reflective style unusual in histories of wars. Sufficient details are provided to locate reasonably precisely most of the men and actions discussed; the skill shown in beating a path through the thicket of policies and campaigns and the clarity achieved in exposition are exemplary. The text is not cramped: the art of knowing what to leave out is enviably on view. Here and there, no doubt, excisions and interpretations may raise questions: Does the Dieppe raid rate no more than this brief allusion? Is the tragic tale of the destruction of the Anglo-French entente complete without mention of Mers el-Kébir? Does the discussion of the Finnish government's state of mind in the first round suggest the ambiguity of its attitude from the moment of the Russian attack? If Halder and Marshall are here, should not Alanbrooke be also? Or does the swift dispatching of Darlan's assassin convey the involvement of this brief act in the vicious power struggle in North Africa?

The bulk of Mr. Wright's account is concerned with civilian responses to the war in the realms of science and technology, economics and psychology, industry and letters. Here particularly one notes his wide acquaintance with much specialized literature. Here too is evident that unevenness in the basic scholarly study of the war to which his preface refers. For instance, the existence of the British official histories permits his discussion of the home front in the United Kingdom to be more secure and authoritative than the corresponding discussions relating to Germany, Italy, or Russia. Despite this qualitative and quantitative imbalance in the literature drawn on, he has skillfully avoided obvious disproportions in his examination of the warring societies. The overall point of view is that normally called liberal; the judgments advanced are moderate and not unexpected. Without ignoring the Hamburg and Dresden episodes, he holds that "one fact seems indisputable: that in the twentieth-century surge toward savagery, the Nazi establishment far outdid all the rest." Refusing to see in the "new order" a foreshadowing of united Europe, he remarks of the German people's situation that in wartime "only the rare individual" is ready to confront "the perils and ambiguities" of opposing the state; and, remote as its outlook was from that of the European resistance movement, he takes the elitist, Christian resistance in Germany seriously.

Discussing the economic effort of the belligerents, Mr. Wright insists upon not only the failures of both the French and the British in the early days, but also the misconception of the task by Hitler and his collaborators. The immensity of the

effort finally put forward by all the principal contestants is held to be one of the great achievements of the war, whatever the ultimate costs. As for psychological warfare, he finds the record a mixed one of spectacular successes and hopes unfulfilled, concluding that while too much was expected of propaganda, it could nevertheless "speed and shape" the war's outcome. For the scientists, of course, the war years offered a series of major successes, particularly in Great Britain, where the civil and military authorities worked closely with them, a situation that had no exact counterpart in Germany. In this section, perhaps more than in others, one notes the lack of material relating to the Russian experience and a certain emphasis on the British evidence. In both these areas, psychological warfare and warfare science and technology, the judgment is that the fundamental contribution and consequences of these six years will emerge only with the passage of time.

The review of war aims and of the breakdown of the "grand alliance" is detached and forbearing. No comment is made on the Eisenhower-Marshall policies (if that, rather than strategy, is the word) in the spring of 1945. Determined readers may conclude that a position is taken on the origin of the cold war, but there is only a hint of the lively controversy that continues. The author is clearly more concerned to see the war from the perspective of developing European society. His view is that, beyond the multiple national rivalries and collisions, Europe was making its way through a "continuing international civil war": parochial conquests or defeats aside, the goal was the preservation or renovation of society. In a sense, Hitler was an agent of destruction clearing the way for an all-out contest for European reconstruction that had threatened only distantly since 1917. This "more profound conflict" is destined to be waged in a Europe made more lawless by the war, more confused in its values, less rational, more savage, more tolerant of autocratic power, and with wounds of the spirit as yet undetermined in nature and magnitude. Against this grim tally is measured the fact that civil society, however distressed, nowhere succumbed to the military (particularly not in Germany, although the developing nature of the Nazi regime may blunt this point). And in a brief epilogue the European Cassandras are judged to have underestimated humanity's "stubborn capacity to scramble back from the pit into which it has been cast by its own follies."

The writing is direct, graceful, illuminated by characteristic sudden flashes of irony. There is a full, partly critical bibliography. Although of uneven quality, the photographs are representative and dramatic. Inevitably the book invites comparisons with Henri Michel's recent two volumes concluding the Halphen and Sagnac series. Mr. Wright's book is briefer (perhaps less than one third the length) and broader in scope. Its comprehensive approach will recommend it to a larger audience than M. Michel's somewhat traditional execution of the same task. *The Ordeal of Total War* is not only a worthy conclusion to the Langer series, it is far and away the best, the most thoughtful, the most comprehensive, and the most civilized single account of those six not very civilized years.

University of Toronto

JOHN C. CAIRNS

AFTER VICTORY: CHURCHILL, ROOSEVELT, STALIN AND THE MAKING OF THE PEACE. By *William L. Neumann*. (New York: Harper and Row. 1967. Pp. xii, 212. Cloth \$5.95, paper \$1.95.)

PROFESSOR Neumann describes his book as an attempt to examine the process of peace-making, not an attempt to indict or judge the Allied leaders. He devotes as much

time to the conduct of the war, however, as he does to peacemaking. The book constitutes a clear survey of wartime relations between the Allies, rather than an attempt at major reinterpretation. It is, in fact, a substantially improved version of the author's earlier *Making of Peace 1941-1945* (Washington, 1950). The improvements are undoubtedly due to a wider perspective in time and the greater abundance of source materials. Unfortunately the author pays little attention to matters of war and peace in the Pacific. This lack is regrettable, since he has done considerable research in US-Japanese relations.

After a survey of European morality of warfare and peacemaking prior to World War II, the author discusses the divergent national aims represented by the respective leaders and the means by which they attempted to realize these aims. He stresses Roosevelt's reluctance to insist on final and specific agreements during the war, his acceptance of unrealistic and vague pronouncements concerning war and peace aims, and his reliance on good will and good fellowship instead of hard negotiations. More seriously, the author charges the president with not having clearly thought out the basis for a postwar world and gives as an instance Roosevelt's concept of great power policemen versus the UN. He further charges Roosevelt with having ignored the education of the American people to the need for compromise. The Soviets, on the other hand, insisted on specific and binding agreements in such matters as territorial changes and reparations.

The texts of various wartime protocols are included—from the Four Freedoms to the Potsdam Agreement—and at the end the author includes a brief bibliographical essay. The book is marred by a number of serious proofreading errors, several of them important dates, which will unfortunately lead to confusion. Despite these minor shortcomings, however, this book will be of value to the professional historian and the interested layman. It will provide a sound basis of information for the study of postwar European history.

Washington State University

FREDERICK DUMIN

SOUTHWARK AND THE CITY. By *David J. Johnson*. (New York: Oxford University Press for the Corporation of London, 1969. Pp. xiv, 441. \$8.75.)

THE Corporation of London has underwritten a work of impeccable scholarship and presented it in a distinguished format. Free of the defensiveness that has often characterized the Corporation's official publications, David Johnson's book, with imperturbable professionalism, depicts the relationship of the City of London with Southwark in all its muddle. The result, if far from flattering, absolves the City from some of the more flamboyant charges of exploitation that were ignited by late nineteenth-century reformers.

The principal source for this history is the Corporation records, and the author effectively analyzes from them the motives for the City's administrative lassitude south of the Thames. The City of London obtained charter rights to the southern terminus of London Bridge in moments of royal weakness: tentatively under Edward III and Henry IV and more comprehensively under Edward VI. The aim of the City governors was to ensure order and to gain some economic supervision. Once these were minimally secure, the City had no further interest in the area. Its denizens were left to be taxed by Surrey officials; they were only nominally represented in the councils of the City by first the greenest and then the most superannuated alderman; and the most effective government they obtained was in manor courts under the

City's steward. Their most pervasive governance throughout was self-generated in the vestries.

What is missing from the book is a satisfactory explanation of how this bizarre congeries of jurisdictions was allowed to persist over the centuries. This lacuna exists because Southwark itself is not the subject of the work. There are glimpses of divisions among the inhabitants in the seventeenth century and even more clearly in connection with the late nineteenth-century reform efforts. The chapters on "The Control of Economic Life" and "Local Self-Government" do something toward describing the life of the people. But until the aims of the various persons and interests within Southwark are carefully delineated, we will be left only to boggle at such a caricature of public order.

Dr. Johnson gives a full and meticulous account of the origins of "the Borough," the conditions of the grants of jurisdiction to the City, and the offices of the chief City administrators—although almost nothing is said about the M.P.'s because they were not subject to the City of London. All this will help to clarify those baffling subjects for the students of London history. But the work has the ultimate defect of its virtues. While it is a punctilious account of administrative relationships, it is not a work of insight into the broader issues of urban history.

New York University

JAMES E. FARNELL

SPECTACLE, PAGEANTRY, AND EARLY TUDOR POLICY. By *Sydney Anglo*. [Oxford-Warburg Studies.] (New York: Oxford University Press. 1969. Pp. viii, 375. \$11.75.)

SYDNEY Anglo has written an engrossing, if heavy-going, iconographic study of early Tudor pageantry. The book is a copious description of golden castles, complicated cosmic mechanisms, celestial palaces, classical and biblical scenes and figures, fire-spouting dragons, and long-winded orations, all of which had intricate allegorical meaning and were intended to dazzle the eye, shape the mind, and sustain the Tudor dynasty.

The political and social significance of this pomp and circumstance is analyzed with great care, especially Henry VII's first triumphal progress in 1486 and that "supreme masterpiece of English civic pageantry," the marriage celebrations in 1501 for Prince Arthur and Catherine of Aragon, when the new Tudor crown was consciously seeking to construct its political and historic image. Anglo sees the civic and political displays between 1485 and 1559 as an artistic unit, the accolades going to Henry VII and Elizabeth for imaginative creativity while most of Henry VIII's reign, for all its exaggerated showmanship, is dismissed as derivative, tawdry, and dull.

As a scholarly portrayal of a vital and colorful aspect of Tudor life, the study is splendid; as an analysis of pageantry as an art form within the social and psychological context of the age, it is less successful. One of the most difficult problems confronting the historian is the relationship between artistic quality and social and political functions. Why the baffling contrast between Henry VII's successes in 1486 and 1501 and the lavish but second-rate performances of his son? Or why the unexplained revival under Elizabeth when, significantly, the queen was seeking, as her grandfather had done to dramatize the promise of a new reign by contrasting it to the disunity and sorrows of the old? The author is in fact more an art historian than a social historian, and only in passing does he discuss political pageantry in connection with the mental habits of the century. State spectacles in order to be really effective political weapons must speak in

symbols that are meaningful to a given audience—in Henry VII's case, a non-reading public that was steeped in a spoken folklore and accustomed to viewing society and human actions in terms of Platonic ideals. It was more than coincidence that the artistic decline of the pageant as an art form was most pronounced during the crisis of the Reformation. Not only was the printing press found to be a more useful instrument of social control over a public decreasingly dependent upon visual stimulation to excite its imagination but also pageantry seems to have momentarily gone out of style—for instance, why the decline of interest among American undergraduates in homecoming parades? That Sydney Anglo's treatment of Tudor pageantry suggests broader cultural and historical problems than are actually discussed is a measure of the importance of the subject and the high standard of the author's scholarship.

Northwestern University

LACEY BALDWIN SMITH

THE REFORMATION PARLIAMENT, 1529–1536. By *Stanford E. Lehmborg*. (New York: Cambridge University Press. 1970. Pp. xi, 282. \$11.50.)

THE clear accomplishment of this book is that it provides a readable account of the seven sessions of Parliament that created the Church of England. In the course of so doing, Professor Lehmborg also gives a good narrative of the activities of Convocation. Whatever analysis there is stands dovetailed into the sessional history, apart from the first four chapters, where attention is given to the forces within England that necessitated the convoking of Parliament and their impact on its composition and procedures. The author's heavy indebtedness to the work of Professor S. T. Bindoff's section of the History of Parliament Trust is acknowledged throughout in the notes. And it is equally clear that telling use has been made of the manuscripts in the House of Lords Record Office, state papers and drafts of bills in the PRO, and the vital Canterbury Province Convocation materials now in the Bodleian Library.

The main thesis of the work is simple enough. Had the "Reformation Parliament" not engendered the Anglican Church it still would have deserved its name, since it passed a barrage of statutes addressed to social, economic, legal, and administrative problems, the collective weight of which had no rival in its own era or for centuries after. In seeking to oblige our consent to this view, Professor Lehmborg treads a path marked out by Professor Elton and by Pollard and Froude. In the main, his work supports the broad outline of Elton's insistence on the importance of the 1530's, although not necessarily the notion that a "revolution in government" accompanied diverse reforms.

So far so good. Where the book creates doubts, they derive from what I can only call a lack of "feel" and interpretive strength. The treatment of Hunne's case is badly flawed. It is not clear that franchise reform in 1430 widened the electorate. The view taken of Wolsey and internal church reform takes inadequate account of recent work. Bias seems to intrude in the treatment of Pole and Cranmer and in the stress put on the Chapuys reports and the anonymous *Life of Fisher*, as well as on the alleged vendetta against the Nun of Kent and her followers. More important, the treatment of Cromwell's role in the Dissolution involves too much supposition and some lack of attention to evidence—for example, on the matter of the relationship between numbers of inhabitants in lesser houses, their *valor*, and irregularity of life. And throughout there is no effort made to pull together good material on opposition in Parliament, procedure, committee workings, and other matters vital to students of representative assemblies in general, as well as the Reformation Parliament. This decision to let things

stand in their sessional context weakens the contribution made to our understanding of the politics of reform.

University of California, Los Angeles

ARTHUR J. SLAVIN

THE MOMENT OF POWER: BRITAIN'S IMPERIAL EPOCH. By *Donald C. Gordon*. (Englewood Cliffs, N. J.: Prentice-Hall. 1970. Pp. viii, 178. Cloth \$5.95, paper \$2.95.)

THE British Empire was not monolithic and coercive, Professor Gordon tells us, but polycentric and permissive. Today, accordingly, more and more of the Empire's historians concentrate on its regional and national units. What was never unified in reality is less and less often the unit of historical study. But *The Moment of Power* takes up the Empire as a whole. If one were to ask, what importance does the study of British Empire history retain after the area specialists have done their work, one could find the answer in this book.

It is not a work of original scholarship, but neither is it a textbook. Professor Gordon has read widely and wisely in the secondary literature of the Empire. He has culled the classics of the subject, and he is also up-to-date. His unifying theme is power, different chapters taking up the roots of power, its limitations, apparatus, economics, arrogance, and decline. These are thoughtful essays, covering a great deal of ground.

The interpretations are balanced throughout, to the extent that some readers might wish a stronger and clearer position from the author. To an excellent critique of Lenin's theory of imperialism, for example, he adds a qualifying statement of the reality of economic imperialism. At several points, too, he seems to side with the nationalist critics of Britain's exploitation of colonial economies; then he qualifies this stand by demonstrating the inherent economic limitations of new primary producing regions. On the question of whether colonial rule was an effective modernizing agency and whether modernization itself was desirable, he also balances his statements on both sides. But these are great questions, about which there is no consensus. The author has served students of history well by raising these questions in such a clear and up-to-date summary.

State University of New York, Stony Brook

JOHN A. WILLIAMS

THE COURT AND THE COUNTRY: THE BEGINNING OF THE ENGLISH REVOLUTION. By *Perez Zagorin*. (New York: Atheneum. 1970. Pp. xiv, 366. \$10.00.)

THIS is an ambitious and important book. Zagorin is not afraid to tackle a big subject and offer his own interpretation of the origins of the Puritan Revolution. His book combines sophisticated analysis with skillful, tightly controlled narrative, distilled from his reading of some of the most important recent monographs and from an impressively wide-ranging grasp of the original sources. By focusing on the half-century before the civil war, the book achieves greater depth than most of the recent studies of the whole century; by going back to the reign of Elizabeth I, it has more scope for interpretation than those that deal only with the period of revolution itself.

The interpretation is multi-causal, not doctrinaire. Zagorin is too conscious of the complexity of the historical process to imprison himself in a theoretical framework, whether Whig, Marxist, or otherwise. His organizing principle is stated in the title: exploration of the Court-Country antithesis already made familiar by the work of

Trevor-Roper and others. In Zagorin's hands the Country, to be sure, becomes something more convincing than Trevor-Roper's mob of office-seeking backwoodsmen striving to recover their fortunes in an age of ruinous inflation. Eliot and Pym led an opposition movement based on principles—conservative and perhaps anachronistic in their view of the constitution, but nevertheless united by a more or less coherent ideology and moved by something more elevated than mere greed for place. As a study of the growth of an opposition movement and its gradual transformation into a revolutionary one, the book demands serious attention.

Zagorin is, in fact, rather better on the Crown, however, than on its opponents. His analysis of the Court is brilliant, and he effectively weaves an account of the collapse of its morale into the later narrative chapters. On the Country he is somewhat less satisfactory, and there is indeed a serious gap in his treatment. Part of the trouble stems from his failure to pursue the implications of one of the several meanings of the word, which he discusses in his second chapter. As Zagorin rightly points out, the term was already acquiring the political significance it commonly held during the following century—the claim to represent the public interest of the whole country, the whole kingdom. But the older meaning, synonymous with county, also survived and would have had a more immediate resonance for most of the members of this Country opposition. Zagorin's perspective is national rather than local. He therefore does not seem to recognize how completely the outlook of many seventeenth-century Englishmen was still bounded by the horizon of their shire. When the members gathered at Westminster in 1640, much of the unanimity of the Country stemmed from their conviction that in the previous eleven years the integrity of their local communities—of their "countries"—had been violated by the centralizing policies of Charles I and his council. Zagorin treats the Country as a national political movement with national objectives. This was true only to a limited extent: for the leaders, perhaps, the movement was national, but for most of their followers it was still a composite of attitudes demanding decentralization against the Court. To ignore this is a curious omission for an author who notes (p. 4) that the one feature common to nearly all the European revolts of this period was protest against centralizing monarchy.

The "county community" concept has been most convincingly argued by Alan Everitt in his *Community of Kent and the Great Rebellion* (1966). This book was apparently published (rather oddly) too recently for Zagorin to make use of it. Still, Everitt's emphasis on the local community had already been anticipated, less sharply, by other writers. Zagorin is certainly entitled to believe that it is a mistake to exaggerate the localism of the Country, but if so, the point should surely be argued. In fact he does make use of T. G. Barnes's study of Somerset, in which the essence of the "county community" thesis is clearly visible. Sir Robert Phelips, the leading political magnate of that county, is indeed a fine example of the complexities of the Court-Country antithesis. Phelips appears briefly in Zagorin as a leading Country parliamentarian (though he is indexed under the wrong first name), but his role as political boss in his shire is not mentioned. There were many who, like Phelips, displayed vocal parliamentary opposition to malignant councilors coupled with assiduous dedication to carrying on the king's government as J. P.'s and D. L.'s in their counties. By 1640 ship money and an excess of supervision by the council drove them into revolutionary courses. But to see them before 1640 exclusively in terms of opposition is to get the emphasis wrong. In the end, of course, they found that as a centralizing force the little finger of Parliament bore more heavily than anything the king had done, and so they came round to monarchy again, hesitantly in 1648, enthusiastically in 1660.

Even with Zagorin's definition of the Country as an opposition movement, the localist aspect deserves greater attention. All the other issues that produced the great confrontation are admirably elucidated. Without saying anything particularly new, Zagorin brings out the crucial importance of foreign policy. He has a sensible chapter on Puritanism and drives one more nail into the coffin of Gardiner's thesis that religion was the crucial factor on which the Long Parliament divided in 1641-42. He is sound on urban and economic matters, though on the urban opposition he does not really go beyond Valerie Pearl's study of London and makes no use at all of Roger Howell's recent work on Newcastle. The insistence on the varied motives of the members of the opposition is wise and judicious. We see these essentially conservative, legalistic reformers gradually discovering the new principle of loyalty to the state rather than to the person of the king and then (in Strafford's trial) digging the pit for their own later downfall by appealing to necessity rather than positive law for justification.

Few historians have the courage to work on so large a scale or to combine, as Zagorin does, breadth of interpretation with conscientious mastery of detail. It is the more to be regretted that he ignores one of the most important issues that defined the antithesis between the Court and the Country.

Brown University

DAVID UNDERDOWN

THE PURITAN REVOLUTION AND EDUCATIONAL THOUGHT: BACKGROUND FOR REFORM. By *Richard L. Greaves*. (New Brunswick, N. J.: Rutgers University Press. 1969. Pp. viii, 188. \$7.50.)

IN his foreword to the book under review, Richard Schlatter points to its timeliness in that the more radical educational reformers of the English Interregnum advocated sweeping educational revolution as part of a general revolution. These reformers proposed plans for a system of universal education for all social classes; they insisted upon "relevance" in terms of utilitarian criteria; and they challenged existing curricula, educational controls and organization, and the means of educational financing. In terms not unfamiliar to the present day, they demanded that education be refashioned as an instrument for creation of the good society. Similarly, they sought to wrest educational controls from the "establishment," which, in turn, looked upon the existing, traditional system of education as a bulwark of law and order.

Professor Schlatter is correct. The parallels are remarkable. Is it not possible that the present revolt in education is an unconscious revival of grandiose ideals that, like so many of the social utopias envisioned during the "Century of Genius," are still unrealized—another instance of the unquenchable power of ideas in human experience? If so, the analogy is yet another illustration of the germinal qualities of the rich revolutionary ideology of the Puritan Revolution.

But Professor Greaves's book is not concerned with prophecies. Rather, it is a solid, admirably annotated study, with revealing analysis and discrimination, of a body of educational literature stemming from English socioeconomic reformers of the Interregnum. With the exception of John Milton's works, nearly all of this literature has been consigned to oblivion, probably because of the harsh and unjust criticism the works of English sectaries have received from the time of Lord Clarendon to the present day. Yet anyone who steeps himself in the pamphlet literature and newsheets preserved in the Thomason Collection in the British Museum is unlikely to conclude that the works of such men as Hugh Peter, Edward Burrough, William Petty, William Dell, Jan Comenius, John Drury, Samuel Hartlib, William Sprigg, Gerrard Winstanley,

Seth Ward, John Webster, and even that noisy promoter of popular education, Balthazar Gerbier, were the babblings of "ignorant critics" and wholly irresponsible fanatics. To be sure, there were other pamphleteers who were both, but even they deserve a critical reading and an effort to explain and evaluate their significance. Professor Greaves has supplied this want and brilliantly analyzed the results.

The limitations of space in this review preclude a lengthy account of Professor Greaves's thesis in detail. At base, in keeping with the times, much of the literature is concerned with man's spiritual estate, marked by violent anticlericalism. It is also strongly tinged with Baconianism, or with that concern for utilitarianism that so frequently has become a corollary of the study of Verulam. For the poor and oppressed—and those who felt oppressed—utilitarianism became a goal of educational reform, as was so often demonstrated in the dissenter academies of a later age. Basically, however, Professor Greaves interprets the differences between Anglican, Puritan, and sectary as differences, or varying shades of difference, in the epistemology of the various groups. Anglicans seldom gave credence to the role of the spirit; Puritans tended to be rationalists, though they clung to an irrational belief in the power of the spirit; and sectaries held that the spirit could reveal knowledge, both sacred and profane, to the poor as well as to the rich, to the uneducated as well as to the formally educated. To the sectaries, and to many of the Puritans, education was considered the principal means to better the condition of the poor. Puritans often found themselves in an embarrassing situation in that they desired to reform education and improve the condition of the poor, although they could not go as far as the sectaries (who professed similar objectives) in rejecting traditional educational institutions and in relying so completely upon the powers of the spirit. Thus, while the sectaries were anticlerical, antiprofessional, and generally antiestablishment (to borrow a term from a later age), the Puritans were caught in a position less wholly opposed to existing institutions. However, as Professor Greaves argues, the sectaries were not, as they have so often been accused of being, uniformly anti-intellectual in their attitudes. Rather, as they sought to destroy existing educational institutions as the bastion of privilege, they also envisioned a new society in which learning would become the property of every man and professionalism would be abolished. Every man, by means of popular and public education, together with the powers of the spirit developed within himself, could be his own minister, his own lawyer, and his own physician, no longer to be victimized by the professionals in theology, law, medicine, and in education itself. "Their dream of a universally enlightened society," concludes the author, "is our heritage." Has it also become our cross to bear?

Illinois State University

RAYMOND P. STEARNS

THE IRISH CATTLE BILLS: A STUDY IN RESTORATION POLITICS. By Carolyn A. Edie. [Transactions of the American Philosophical Society, New Series, Volume LX, Part 2.] (Philadelphia: the Society. 1970. Pp. 66. \$2.50.)

THE author of this short monograph promises to overcome the myopia seemingly congenital in the genre, but fails to live up to her promise. Professor Edie begins by arguing, probably quite correctly, that the Irish cattle bills raised "serious and difficult questions of politics, of law and of economics," and that, being crucial for "English policies toward her Irish and colonial dependencies," they are "matters of consequence in English, Irish, and even American history." The argument remains one by assertion only, however, as the body of the work consists largely of reportage of parliamentary

debates and surrounding correspondence, followed by conclusions of a most general and unsurprising nature.

Those readers in particular who are led by the book's main title (and its green cover) to expect a significant contribution to Irish or Anglo-Irish history will be disappointed; one major conclusion, that the cattle bills of the 1660's "mark the genesis" of a new English policy toward Ireland, seems based on a too-narrow reading of the 1621 debates (excluding those in response to Irish petitions, for example) and a decision to ignore recent literature in the field. Similarly, the book's economic critique is, if anything, microeconomic, and there is little sense of Stone, Laslett, or Namier in the socio-political matter that dominates the book, so that factions and schemers seem arbitrarily or simplistically motivated. In short, Professor Edie ignores contemporary literature and contemporary concerns in most of the areas she treats. The book does bring into print more materials relating to the Restoration parliaments and is virtually free from printing errors.

Prescott College

EUGENE T. KELLY

THE CHARTER CONTROVERSY IN THE CITY OF LONDON, 1660-1688, AND ITS CONSEQUENCES. By *Jennifer Levin*. [University of London Legal Series, under the auspices of the Institute of Advanced Legal Studies, Number 9.] ([London:] University of London, Athlone Press; distrib. by Oxford University Press, New York. 1969. Pp. 119. \$4.95.)

THIS fine addition to the University of London Legal Series traces in detail the charter controversy in the city of London. Mrs. Jennifer Levin, a lawyer-historian, has combined a deep knowledge of law with a keen insight into the historical implications of this conflict. A conflict, incidentally, that touches on the eternal and modern issue of government intervention and pressure. The king would not forfeit the charter if the city submitted to certain conditions. Should the city submit or fight? London submitted but still argued that a corporation franchise could not be transferred. As a corporation has only those powers given to it by its charter, how can it validly surrender that charter unless such a power is expressly given? This question, of course, raises the wider issue of *ultra vires* in general. From October 1683 to October 1688 London was governed by a royal commission.

What were Charles's and James's motives? Was it a matter of the king's "gently" laying hands on the city? The boroughs had been roundhead strongholds during the Civil War and were traditionally Protestant, so it was from this source that Charles faced his strongest opposition. The Crown could deal with the counties through the lord lieutenants. It was the boroughs that caused the real difficulty as the majority members of Parliament were returned by them. The case was of immense political importance, and the surrender of the boroughs, which it precipitated, was one of the major causes of the Revolution of 1688.

Other corporations surrendered their charters on flimsier grounds than did London. Thus it is hoped that this excellent study will provide a stimulus for additional monographs. Mrs. Levin has perhaps overemphasized the power and influence of Shaftesbury. A more detailed bibliography would be helpful, but no legal or political historian should ignore this work.

University of Toledo

RICHARD E. BOYER

THE ORIGINS OF POLITICAL STABILITY: ENGLAND, 1675-1725. By J. H. Plumb. (Boston: Houghton Mifflin Company. 1967. Pp. xviii, 206. \$6.00.)

THE GROWTH OF POLITICAL STABILITY IN ENGLAND, 1675-1725. By J. H. Plumb. [Peregrine Books.] (Reprint; Baltimore: Penguin Books. 1969. Pp. 207. \$2.25.)

TORY & WHIG: THE STRUGGLE IN THE CONSTITUENCIES, 1701-1715. By W. A. Speck. ([New York:] St. Martin's Press. 1970. Pp. xi, 164. \$9.75.)

AFTER some years of comparative neglect, the generation following the Revolution of 1689 in England has recently attracted the attention of a large number of historians, and published work is now beginning to flow. Much of this has been inspired by the writing and teaching of the Cambridge historian J. H. Plumb, and it was a fitting tribute to him that he was invited to deliver the Ford Lectures at Oxford in 1965. It is particularly fortunate that he turned to this period in which he has for so long been at home, for the result, in the printed version of the lectures, is a book of the first importance.

Professor Plumb sets out to "clear up some of the confusion" that has developed in the field, particularly over the nature of party, and to tackle the dominating problem of the period: why the strife of the seventeenth century gave way to the settled, assured, unbroken calm of the mid-eighteenth-century political world. In the course of his six lectures Plumb analyzes the forces that underlay the instability of the seventeenth century and those that were making for stability in the eighteenth. These are in turn isolated, described, analyzed, and then fitted into an explanatory scheme of compelling force. Some are given proper emphasis for the first time. The influence, for example, of an electorate that expanded rapidly in the seventeenth century and of the frequent elections under William and Anne have not before received the attention he gives to them and that clearly they deserve. Plumb sees the electorate as a disruptive force lying behind the "rage of party" and the fluctuating fortunes of governments, and it is central to his explanation of the growth of stability that the electorate was countered and controlled by two developments of fundamental importance. The first, the growth of oligarchy, was a product of the feverish and frequent electioneering, particularly of its mounting costs, which drove small men from the field and increasingly concentrated electoral power in fewer and fewer hands. In addition, patrons and candidates were thrown into ever greater dependence on the government, for the favors that were expected of them could only be provided by the court or the departments of state. The government in turn was increasingly in a better position to satisfy these needs. In a chapter of striking brilliance and originality Professor Plumb outlines the growth in the scope and size of the government after 1689—a response, principally, to the demands of war in Europe—that provided the means by which electoral patrons could be more easily tied to the government. Thus, as the power structure narrowed, it necessarily centered on the government, and there was produced a steady "tidal force" encouraging stability. There was nothing automatic, however, about the process; indeed, Plumb emphasizes that the growth of the government and of oligarchy contributed to rather than diminished political turmoil in Anne's reign, and that this expansion underlay the struggle between the Whig and Tory parties. It was only after 1714, when the political temperature was lowered, that the potential could be realized and stability achieved. That it came then was due to the genius of Robert Walpole who harnessed the forces making for stability in government and built a regime that by its longevity created the secure and stable world of the mid-century.

Walpole's success was founded on the destruction of the Tory party after 1714 and the consequent triumph of one political group. Throughout the book and especially in the chapter on the "rage of party," Plumb places emphasis on the reality of the party struggle, particularly in Anne's reign, as a conflict between Whigs and Tories over principles as well as power, and as a conflict that took place in the constituencies as well as at the center. This is the subject of Dr. W. A. Speck's important essay. His book is a study of elections and the electorate in Anne's reign based on a formidable range of source material. His central point is that the electorate was deeply concerned with matters of principle and policy and, more important, that a significant proportion were free to register their concern at elections. The existence of a large number of such "floating voters," who could not be bribed or dragooned but who gave their votes to one or another of the parties according to events and the frequency of elections in a period in which issues were hotly debated, combined to produce wild swings in party strength in Parliament. Speck perhaps strains his evidence in order to establish exact party totals following elections, but his central argument is convincing, and he has added an important dimension to the study of party in the reign of Queen Anne.

University of Toronto

J. M. BEATTIE

THE TRIUMPH OF THE LAWYERS: THEIR ROLE IN ENGLISH POLITICS, 1678-1689. By *Michael Landon*. (University: University of Alabama Press. 1970. Pp. 303. \$7.50.)

HISTORIANS have often depicted the common lawyers as the heroes of seventeenth-century English politics. Inspired by the example of Sir Edward Coke, the lawyers led the struggle to place legal restrictions upon the exercise of the king's prerogative. To this end they allied themselves with the parliamentary opposition before 1642 and again after 1678. The Revolution of 1689, which ensured the preservation of the rule of law and regular parliamentary government, thus represented, in G. M. Trevelyan's words, "the triumph of the Common Law and lawyers over the King."

Michael Landon's book attempts to explain the exact nature of the common lawyers' victory. His work focuses on the political activities of seven Whig lawyers—John Maynard, Robert Atkyns, William Jones, George Treby, Henry Pollfexen, William Williams, and Francis Winnington—from their efforts to exclude James, Duke of York, from the throne in 1678 to their legal rationalization of William and Mary's displacement of James in 1689. The author traces the lawyers' opposition to the Crown's *quo warranto* proceedings against corporate cities and boroughs, their legal assistance to harassed Whig leaders, and their criticism of the king's exercise of his dispensing and suspending powers. While in no way overlooking the political "discretion" or opportunism that on separate occasions blemished the Whiggery of Winnington, Pollfexen, and Williams, he does establish a common political outlook among the seven.

Mr. Landon is reluctant to accept Trevelyan's statement at face value. What triumphed in 1689, he explains, was neither the common law nor the common lawyers, but the Whig lawyers' interpretation of the common law. He recognizes, moreover, that certain prominent lawyers gave strong support to the Crown throughout the seventeenth century. Yet the author nonetheless assumes an unwarrantably high degree of political uniformity within the legal profession. He asserts, without any statistical verification, that the seven Whig lawyers represented the political views of a majority of their colleagues. He claims further that all lawyers, including those who defended a broad interpretation of the royal prerogative, considered the law to be the sovereign

power in the state. Certainly all common lawyers respected the supremacy of the law, but they hardly claimed that it rivalled the king or king-in-Parliament for sovereignty. The common law simply regulated the exercise of political power, and the common lawyers' disagreement concerning the nature and extent of that regulation created significant political divisions within their profession.

University of Texas

BRIAN LEVACK

JACOBITE IRELAND, 1685-91. By J. G. Simms. [Studies in Irish History, Second Series, Volume V.] (Toronto: University of Toronto Press. 1969. Pp. xii, 297. \$9.50.)

J. G. SIMMS has established himself as an authority on seventeenth- and eighteenth-century Ireland with a number of articles and short monographs and with his book *The Williamite Confiscation in Ireland 1690-1703* (1956). His new book provides the first reasonably objective study of Ireland under James II and during the Williamite War. Making use of published and unpublished sources, including Danish, Dutch, and French documents, he has put together an account of the military struggles with an explanation of the political and diplomatic forces that caused the war and determined its outcome. The first three chapters describe Ireland's position at the close of Charles II's reign and the political developments between 1685-88, when James II backed Tyrconnell's attempts to wrest control of Ireland from the Protestants. Had James remained king of England, Tyrconnell might have effected a shift of political power to Catholics while still retaining sufficient Anglo-Irish support by protecting Protestant landholders.

The English Revolution of 1688 completely altered the situation. When James landed in Ireland in 1689 he came as an ally of Louis XIV against William of Orange and inevitably found himself the captive leader of the Irish Catholics, despite his English prejudices and desire to win some Protestant backing. Although James's ambivalence and ineffectiveness contributed to his failure, Simms shows that other factors go far toward explaining the subsequent Irish defeat. The Irish army was poorly trained and equipped and suffered from divided leadership. Furthermore French assistance proved inadequate. Both Louis XIV and Louvois viewed the Irish war only as a diversionary and delaying maneuver. Simms is particularly helpful in explaining how the attitudes of the Irish leaders, such as Tyrconnell, Sarsfield, and the French generals, depended upon their differing assessments of a rapidly shifting military situation. William's willingness to offer lenient terms likewise fluctuated. Shortly after the Battle of the Boyne, when he anticipated a quick triumph, he was less generous than later when he recognized the Irish power of prolonged resistance. Though better organized, William's army of Danish, Dutch, English, Huguenots, and Ulstermen experienced internal rivalries and divided councils, but it had a larger proportion of professionals and was better supplied. Irish morale withstood the Boyne defeat, the withdrawal of James, short rations, and false hopes of French aid until the summer of 1691 brought the defeat at Aughrim and the surrender of Galway. Even then Ginkel had slim hopes of taking Limerick before winter set in, but the Irish proved willing to negotiate when he offered them as reasonable terms as William, the English government, and the Irish lords justices were willing to approve. Simms concludes with a brief but careful discussion of the Treaty of Limerick, a subject on which he is an expert. The book is clearly written, has maps of the chief battles, and includes a critical bibliography.

Tulane University

FRANCIS G. JAMES

PARTNERS IN SCIENCE: LETTERS OF JAMES WATT AND JOSEPH BLACK.

Edited with introductions and notes by *Eric Robinson* and *Douglas McKie*. (Cambridge, Mass.: Harvard University Press. 1970. Pp. xvi, 502. \$12.50.)

STARTING his career in the 1750's as an instrument maker at Glasgow University, James Watt became a Fellow of the Royal Society and a wealthy industrialist. His earliest scientific knowledge was picked up in conversation with Joseph Black, then a young professor of chemistry at Glasgow; John Robison, a student, aroused his interest in steam engines. The friendship of the three men lasted until Black died in 1799 and Robison in 1805, and they kept up regular correspondence.

Most of the 260 letters now edited by Mr. Eric Robinson, an economic historian, and the late Professor Douglas McKie, a historian of science, have not appeared in print before, and they form a valuable addition to the published source material of the Industrial Revolution. There is little explanatory matter, so this is not a book for the general reader, but economic and social historians will benefit from it as will historians of science and technology, for the correspondence is concerned as much with problems arising from the exploitation of Watt's inventions as it is with the scientific work of the three friends. The editors have included some letters exchanged by Watt and other correspondents that broaden the scope of the book and provide further evidence of the close relations between scientists and industrialists in eighteenth-century Britain.

The letters are followed by a meticulous transcript of Watt's scientific notebook, which contains the results of experiments on the latent heat and other properties of steam. The editorial discussion must surely have been completed after the death of Professor McKie, an authority on this subject, for it is weakened by an apparent misunderstanding of the function of the separate condenser in Watt's steam engine. Nevertheless, the text is important, for it includes data omitted by Watt from his published account of these experiments, to which, unfortunately, the editors have not referred. The notebook and many of the letters are preserved, with numerous other documents, by Watt's descendants in Wales. Is it too much to hope that this important archive might be deposited in one of the British national libraries?

University College, London

W. A. SMEATON

THE FOX-NORTH COALITION: CRISIS OF THE CONSTITUTION, 1782-4.

By *John Cannon*. (New York: Cambridge University Press. 1969. Pp. xiii, 275. \$10.50.)

JOHN Cannon has produced a meticulously researched and tightly reasoned account of the most severe and prolonged constitutional crisis in eighteenth-century England. His study is particularly welcome since it fills what has long been the most conspicuous gap in eighteenth-century political history. He has much to say that is new: for example, Pitt's full complicity with the king in the fall of the Coalition is clearly established from new evidence. The king comes in for rough treatment in this book, as does Shelburne; but while Cannon's judgments are harsh, they are carefully argued and will doubtless tip the scales in discussions of this episode for many years to come.

But while this is a distinguished book given Cannon's chosen level of analysis, I must criticize it for a lack of perspective, which tends to distort many of its major conclusions. British political life was undergoing the most profound and rapid changes in 1782-84. While Cannon seems to agree with such a thesis, and while he notes some of the features of change such as the emergence of organized party, he does so only

fragmentarily and without clearly distinguishing what was new from what was old. In part this deficiency results from his not having examined manuscripts of critical importance: most particularly the Blair Adam Manuscripts and the Aske Manuscripts. More importantly, how can one evaluate the pressures operating upon men like Pitt or Fox without giving full weight and attention to their involvement in the movement for parliamentary reform? I find it astonishing that a book on the politics of 1782–84 contains only two brief allusions to the activities of Christopher Wyvill.

I would also have liked—for once—a more sophisticated explanation of why lesser gentry and townsmen in such numbers throughout Britain should have been so suddenly aroused to such unprecedented indignation by so relatively remote an issue as a measure for the governance of India. While Cannon's explanation of the events of early 1784 is easily the best to date, it still is far short of the mark. In finding more adequate explanations for political upheaval, we shall have to tread ground not traditionally congenial to political historians, namely, the relationship between social and political change in late eighteenth-century Britain.

Sir George Williams University

DONALD E. GINTER

THE LIVERPOOL & MANCHESTER RAILWAY PROJECT 1821–1831. By *Robert E. Carlson*. (New York: Augustus M. Kelley. 1969. Pp. 292. \$15.00.)

THE story of the Liverpool and Manchester Railway, "the world's first successful steam locomotive railway," is familiar to historians of nineteenth-century Britain. Nevertheless, Professor Carlson succeeds in adding much that is new by utilizing hitherto unexploited sources and by concentrating on the origins and development of the project for the railway.

Only the last two of the nine chapters deal with the construction of the railway, the Rainhill trials, and the early operation, topics to which attention has hitherto been largely directed. Carlson's major emphasis, as shown by the dates of his study, 1821–31, is on the initiation of the project, its promotion, and eventual authorization by Parliament. To this end he uses sources that have not been fully investigated by previous writers: contemporary newspapers and periodicals, and most importantly, parliamentary records, particularly the reports and minutes of the Commons and Lords committees on the 1825 and 1826 applications for the bill authorizing construction of the railway. These sources reveal the difficulties encountered by the railway's sponsors, particularly the opposition of the competing canal companies, the landowners through whose lands the railway would run, and even the cities of Liverpool and Manchester themselves, which feared the unknown dangers of these strange engines chugging into town.

When the original attempt to obtain an enabling act failed in 1825, the railway modified its plans and co-opted some of its opposition, so it received approval of its plans the following year. Carlson carefully analyzes the act of 1826, which became the pattern for later railway legislation, and shows how it represented a turning point in Parliament's response to the new age of steam.

Carlson tells how George Stephenson wrecked the chances for the passage of the 1825 Act by his inept testimony before Parliament. His account of Stephenson's being re-hired by the railway company and successfully building the railroad should be supplemented by L. T. C. Rolt's account of these same episodes in *The Railway Revolution: George and Robert Stephenson* (1962).

All told, Professor Carlson has provided us with a well-written survey of a crucial

development in social, economic, and technological history. His researches are well documented, and there is a comprehensive bibliography. The only bibliographical item I found missing is G. O. Holt's brief but important *A Short History of the Liverpool and Manchester Railway* (2d ed., 1965).

Case Western Reserve University

MELVIN KRANZBERG

THE PAUPER PRESS: A STUDY IN WORKING-CLASS RADICALISM OF THE 1830s. By *Patricia Hollis*. [Oxford Historical Monographs.] (New York: Oxford University Press. 1970. Pp. xvi, 348. \$9.95.)

DR. Hollis poses systematically the important questions about the unstamped radical press, she asks her questions so precisely that they will not admit of vague answers, and she answers them using acceptable methods. The result is an excellent book, far superior to other work on the subject. Part of its excellence is a range wider than the subtitle would suggest. The first third of the text is devoted to placing the pauper press in the context of nineteenth-century ideas about education, governmental concern with revenue, problems of public order, and middle-class attempts to reduce the "taxes on knowledge."

But the focus of the book is very much on working-class radicalism. It provides us with a picture of the organization of the pauper press: the financing of the papers and their production, distribution, profits, and relationship to radical unions and associations in London. An analysis of the street vendors prosecuted by the authorities in London gives added dimension to the account of the distributive system and helps to explain the vendors' participation. An analysis of the sentences they incurred shows that justice was surprisingly even-handed. The book is packed with data that are presented in ways that not only illuminate the subject but also are valuable to other historians working in the period: a graph showing incidence of prosecution for selling the unstamped in London over six years, a map and table showing the provincial penetration of the radical unstamped, and capsule biographies of the principal producers of the London pauper press.

Two important contributions of the book are a discussion of class relations in the attempts to repeal the stamp duties and a discussion of the ideology of the radical unstamped. Dr. Hollis argues that the existence of the pauper press did not mean that middle- and working-class radicals were divided as E. P. Thompson has suggested. She points out that one-half of the unstamped in circulation put forward an ideology that was acceptable to middle-class radicals and that the agitation brought leaders of the two classes in London "into a hesitant but genuine alliance." Furthermore, she shows the ways in which the radicals of both classes dovetailed their efforts and supported each other's activities.

The place of the unstamped in the development of working-class radical ideology may be judged from a resolution of 1838 of the London Working Men's Association designating the *Encyclopaedia Britannica* and the *Poor Man's Guardian* works of reference and restricting them to the association's reading room. The pauper press—principally the papers with which O'Brien and Hetherington were associated—is credited with establishing a new working-class radical ideology. Dr. Hollis discusses this development in terms of "older analyses" of society by the writers of 1819 who saw the people as oppressed by aristocratic legislation and the established church as compared to a newer analysis of the 1830's, which saw the people as exploited by capitalists and the system of property ownership, for example, E. P. Thompson had

outlined this terrain; Dr. Hollis has thoroughly explored it and has added much to Thompson's work. It is a discussion of importance to others in the field who should be as careful as Dr. Hollis in not overlooking the vigor of the language of oppression because they find the language of exploitation "more appropriate."

University of New Hampshire

THOMAS MILTON KEMNITZ

BRITISH COLONIAL ADMINISTRATION IN THE MID-NINETEENTH CENTURY: THE POLICY-MAKING PROCESS. By *John W. Cell*. (New Haven, Conn.: Yale University Press. 1970. Pp. xiv, 344. \$10.00.)

PROFESSOR Cell's book covers rather more ground than is implied in the title. There is an interesting discussion of the policy of responsible government, as well as an account of the process from which that policy emerged. In addition, there are two substantial case studies dealing with the development of communications between Britain and the East and the annexation of Lagos. This short review will, however, concentrate on the value of the book to the administrative historian.

In his first chapter, Cell shows the Colonial Office at work during the period 1835-70, or thereabouts. His accounts add to what was already known from the work of such scholars as H. Hall, D. M. Young, and R. B. Pugh, correcting the picture in some respects. Cell then goes on to something more original: an analysis of the administrative machinery at the disposal of the governors of the selected colonies. The administrative developments resulting from responsible government—hitherto a neglected theme except in the case of Canada, where J. E. Hodgetts' valuable study is available—are sketched in a firm, clear, outline. Members of the public service, both in the Colonial Office itself and in the colonies, were of course recruited by patronage until the closing years of the period when a system of limited competition was brought in. Yet Cell shows clearly that the results were by no means wholly bad. At the London end, "the senior officials formed an impressive core of assistants" to successive secretaries of state. "Taken as a group they were knowledgeable, industrious, and extraordinarily continuous." Overseas, there was already a colonial service as distinct from a number of individuals each appointed *ad hoc*. Cell analyzes the careers of all colonial governors during the period and finds that "only 91 men . . . served in just one assignment, whereas 168 could be identified as having risen out of the lower ranks of the service." As for quality, Cell concludes "that the colonial service was much less of a 'job,' much less 'corrupt,' than might be supposed from the contemporary novels of a Thackeray or a Trollope." The trouble an ineffectual man in a top position could cause for a ministry normally far outweighed any political advantage arising from his appointment.

To sum up, Professor Cell's book is a welcome addition to the growing list of monographs available to the student of British administrative history in the nineteenth century.

Civil Service College

HENRY PARRIS

VICTORIAN REVOLUTIONARIES: SPECULATIONS ON SOME HEROES OF A CULTURE CRISIS. By *Morse Peckham*. (New York: George Braziller. 1970. Pp. 310. \$7.50.)

MORSE Peckham's *Victorian Revolutionaries* is by turns outrageous and illuminating, a "provocative work" in more senses than the writer of the jacket copy intended.

Most himself when most hyperbolic, Professor Peckham asserts that "the culture crisis of the nineteenth century was the greatest not merely in European history, but in human history," and that we are its anxious heirs. Certain writers within the period—Tennyson, Browning, Rossetti, the pioneering anthropologist Edward Tylor, and Swinburne—managed, like the "Danish Victorian" Kierkegaard, to escape the pre-suppositions of their time. To their various strategies of cultural transcendence Professor Peckham devotes the six loosely linked essays, which, to cite the book's subtitle, constitute his dizzying "Speculations on Some Heroes of a Culture Crisis."

For the generation between the two world wars, the Victorians appeared complacently wedded to the myth of progress. We have succeeded in turning the Victorians on their heads, perceiving doubt where our critical predecessors saw certainty, iconoclasm where they saw conformity, pornography where they perceived only prudery. For our predecessors, the Victorians were contemptible because of their supposed conviction that they inhabited the best of possible worlds; for us, who take a kind of calamitarian pride in living in the worst of worlds, they are the admirable prophets of the impending apocalypse. *Victorian Revolutionaries* pushes to the very frontier of credibility the new, inverted view of the period: the Victorians are great and greatly relevant to us not because of their sanity but because of their terror, because, in short, they are just like us, profoundly anxious, profoundly in crisis. I begin to fear that the "modish" view is as distorted and provincial as the one it has displaced, though biased in precisely the opposite direction.

To this general warning against the "modish" bias of the book must be added a specific caveat concerning Professor Peckham's fondness for digression and startling juxtaposition—"... the great nineteenth-century revolutionaries, Marx as well as Tennyson, Chopin as well as Engels. . . ." He is also guilty of occasional lapses from fact (the young Browning was "bewildered and seduced by immediate success," whereas in truth he was bewildered by thirty years of obscurity), and in at least one instance—when writing on Swinburne as an important political philosopher—Professor Peckham indulges in pure fantasy: "His meaningful meaningless [*sic*] analysis of social structure was more subtle and more thorough-going not only than the primitive efforts of de Sade but also than the infinitely more sophisticated efforts of Hegel, and certainly far more than Marx's."

Admittedly, this is *Victorian Revolutionaries* at its most extravagant, and that extravagance is irritating indeed, if only because one feels that a very bright mind has too frequently substituted a facile and self-indulgent rhetoric for responsible analysis. Yet the book deserves to be read almost in spite of its "brilliance." Professor Peckham unconsciously re-enacts in his pages the tortured crisis of mind he sets out to describe, straining at the limits of language while analyzing language and groping for a new form, neither historical nor literary-critical, to contain his perceptions. At times that effort produces a sleazy sort of Pop metaphysics of culture; at times it yields the genuine article. A very few examples must suffice.

There is, first, the startling novelty of beginning a book on Victorian revolutionaries with the revered laureate whom James Joyce dismissed as Alfred Lawn Tennyson. A generation and more ago in his essay on *In Memoriam* (1936), T. S. Eliot made Tennyson's great elegy accessible to the modern reader in a single sentence: "It is not religious because of the quality of its faith but because of the quality of its doubt." But Eliot could not see beyond the achievement of *In Memoriam* to the still greater achievement of the *Idylls of the King*, which he rejected with a glancing sneer. Professor Peckham rightly recognizes the *Idylls* as a magnificent and despairing

vision of the illusory quality of all human action, including the grand illusion of human perfectibility. The point is not wholly original, but *Victorian Revolutionaries* will carry it beyond the tight little family of Victorian literary scholars. The same capacity to see the familiar or the despised with fresh eyes leads Professor Peckham to take Swinburne seriously, to praise Carlyle's *History of Friedrich II* as a masterpiece, and to find undiscovered riches in the craggy byways of Browning's later verse.

Yet only the essay on Carlyle fulfills the promise of the preface and gives us, fully fleshed, a portrait of the writer in radical and heroic alienation from his culture. Professor Peckham has something of Carlyle's comic-dyspeptic fury, and one wishes he had written more about the author of *Sartor Resartus* and less about the Pre-Raphaelites, who serve him as mere vehicles for "speculations" on the pathology and economics of art collecting. But on Carlyle Professor Peckham is quite splendid, and I feel a certain justice in concluding this review of a book about which I have the severest reservations with a quotation for which I have unqualified praise: "This sense of man's unalterable inadequacy is what gives Carlyle's writings their bite; this is the source, too, of his wild humor, a mingling of contempt and compassion." Nothing I have read comes closer to defining the unique components of Carlyle's grim mirth, a sort of doomsday laughter that finds its occasional echo in *Victorian Revolutionaries*.

Columbia University

JOHN D. ROSENBERG

JOHN STUART MILL. By Alan Ryan. (New York: Pantheon Books. 1970. Pp. xx, 268. \$6.95.)

THIS is the second book in recent years that sets out to establish that there is unity and system in Mill's thought. Professor John M. Robson in *The Improvement of Mankind* (1968) argued for "the central importance of the utilitarian ethic." Now Alan Ryan, in a study that focuses on the structure and interrelationship of Mill's ideas, puts the *Logic* at the core of Mill's philosophy. "The aim of this book is to present Mill as the author of a philosophic system, a system which I shall call 'inductivism.' . . ." These are Ryan's opening words; and what follows is a tightly woven argument to establish that Mill, in his analysis of reasoning from mathematics to sociology, was attempting to apply a consistent mode of scientific proof and explanation. Ryan points out that Mill felt impelled to build such a system in order to discredit the intuitionists, whom he considered a threat to scientific progress, political change, and personal liberty.

As Mill's purpose was reform as well as explanation, it was essential that he formulate a system of rational ethics. Ryan reminds Mill's critics that Mill was aware that ethics was not a science but an "art"—a series of rules and sanctions for the good of all; but Mill insisted that while scientific "proof" in ethics was impossible, rational explanation was both possible and necessary. To meet his own criteria of reasoning and to cope with the intuitionists Mill needed a guiding principle for ethics, but the principle of utility, as Ryan skillfully argues, does not always serve the purpose, especially when it clashes with at least one other basic principle that Mill values—justice.

The goal of life for Mill was not merely a scientifically ordered society, but the free development of individual character. In a refreshing interpretation of *Liberty*, Ryan maintains that it is this quality of life that rational ethics (and, indeed, Mill's whole system) was designed to promote. While few students of Mill have found much virtue in his distinction between self-regarding and other-regarding actions,

Ryan defends it as adequate for Mill's purpose, which was to protect the individual from legal and social coercion in those areas where there is neither intention nor likelihood of harm to others.

Ryan overemphasizes the purely cerebral and systematic quality in Mill, while tending to neglect his emotional and wider intellectual development as well as the political and social milieu in which he wrote. Yet this is a lively and penetrating book, which illuminates some of the lesser emphasized features in Mill's thought. It succeeds in placing the *Utilitarianism* and *Liberty* in the framework of the *Logic*; and, within the scope of the works it treats, it makes a good case for Mill as the author of an inductive system.

York University

SYDNEY EISEN

ADMIRAL OF THE FLEET: THE LIFE OF JOHN FISHER. By *Richard Hough*. ([New York:] Macmillan Company. 1970. Pp. 392. \$8.95.)

THERE are very few admirals who are commonly known by their first names—Jacky Fisher was one of them. This cocky officer was admired and hated by a great number of people at a time when serving personnel were rarely known outside the Admiralty. His use of the press to popularize his programs accomplished this.

Richard Hough has written the first full-length biography of Fisher since Admiral Bacon's study forty years ago. This is a wider work than Bacon's volumes, which were essentially an appreciation and drew upon Fisher's papers and the reminiscences of his contemporaries. Hough uses Bacon's sources, the Admiralty library—a little-used but complete and well-indexed repository—the works of Arthur Marder, and other personal and state papers. He might have made greater use of the official papers at the Public Record Office, yet he has done a fine study of the admiral's meteoric career.

Born in humble circumstances to an army captain's wife in Ceylon, Fisher was poor for a great part of his life. Unlike most of his fellow officers, he depended almost completely on his salary. But aided by much good luck and an uncanny ability to get on with the right people, Fisher steadily progressed. In a career spanning nearly sixty years, he served in the Crimean War, against the Chinese, and as commander of both the North-American and the Mediterranean stations. He fought in Egypt and was at the Admiralty as first sea lord during 1904–10 and 1914–15.

Mr. Hough devotes nearly half the book to this latter period, when Fisher's great reforms were initiated. These included the reduction and elimination of many of the world-wide naval stations that were the basis of the nineteenth-century Pax Britannia. To face the growing German menace, he concentrated the fleet in home waters. His greatest conception was the *Dreadnought*, the first all big-gun battleship. Fisher, more than any other individual, was responsible for the fact that when war began, the navy was modern and ready.

Mr. Hough is certainly fond of the old admiral, but he does admit some faults: ". . . the impatience . . . the violent manner of expression which led him to over-state a case . . . and incense his opponents . . . his readiness to make enemies . . . and his pride in his own excesses of temperament. . . ." Although Hough, like Marder, has covered Fisher's time at the Admiralty, the merit of this book lies in its complete picture of the man, the years of promise and fulfillment.

C. W. Post College

REGIS A. COURTEMANCHE

THE FOREIGN OFFICE AND FOREIGN POLICY, 1898-1914. By *Zara S. Steiner*. (New York: Cambridge University Press. 1969. Pp. xii, 262. \$10.50.)

THIS unusual and penetrating study deals with the period before World War I when the British Foreign Office reached the peak of its modern power. Until the end of Lord Salisbury's tenure of office, there was a deep gulf between the foreign secretary and his staff. Salisbury made his own decisions and expected them to be carried out. To be sure, changes were beginning to occur before 1900, but it was under Lord Lansdowne and especially Sir Edward Grey that the permanent civil servants came to take it as their prime responsibility to advise their chief in ways their predecessors never attempted. And perhaps by chance (since they were recruited from a narrow elite on often irrelevant criteria), perhaps by the deliberate encouragement of the first twentieth-century foreign secretaries, a remarkable group of powerful men came to the fore and set the tone—or tones, for there was often cacophony—of the prewar foreign policy establishment. Charles Hardinge, William Tyrrell, Arthur Nicolson, and Eyre Crowe are described in sensitive and sensible vignettes, and their role in policy making is assessed. If Mrs. Steiner's book had no other virtues, and it has many, it would be invaluable for its abundant demonstration that Hardinge's, Nicolson's and especially Crowe's influence on decision making has been exaggerated. The suspicions of contemporary critics about gray eminences in the Foreign Office, pushing Great Britain toward the holocaust, simply do not stand up under her scrutiny of the records. Sir Edward Grey was indeed his own master and was the final arbiter on foreign affairs, however powerful and eloquent, for example, the memoranda of Sir Eyre Crowe may have been. The distinction between the responsible minister and his civil servants remained. Mrs. Steiner, incidentally, finds Grey's policy decisions a matter for approval; all that she has to say confirms my own less than enthusiastic assessment of both his skill and his wisdom in the years before Sarajevo. Although Mrs. Steiner has some interesting general remarks—the outline possibly for another book—on the influence of diplomats, press, crown, and parliament upon foreign affairs, the heart of her monograph is concerned with the permanent officials in the Foreign Office. She succeeds brilliantly in her intended "portrait of an institution," and succeeds as well in adding a fresh perspective to British foreign policy before the war, not an inconsiderable achievement in what some have recently described as an oversaturated field of investigation.

Rutgers University

HENRY R. WINKLER

THE CHIEF SECRETARY: AUGUSTINE BIRRELL IN IRELAND. By *Leon Ó Broin*. ([Hamden, Conn.:] Archon Books. 1970. Pp. 232. \$7.25.)

WHEN he resigned as Irish chief secretary in April 1916, Augustine Birrell was universally condemned for not having taken measures to forestall the Easter Rising. He had refused to act upon the R.I.C. reports of widespread disaffection and had resisted constant military pressure to suppress the volunteers. Confronted at last with plain evidence of the Rising being planned, Birrell ordered the arrest of the leaders—just twenty-four hours too late. If he erred in underestimating the strength of Sinn Féin, Birrell was not alone: the Nationalist leaders, Redmond and Dillon, equally misjudged the temper of their countrymen.

In Birrell's defense, it might also be argued that premature action against Sinn Féin and the volunteers would have had the inevitable result of increasing their

popularity in Ireland. It was for a similar reason that Birrell had been reluctant to take legal action against the Ulster leaders in 1913-14, notwithstanding the treasonable nature of their provocation.

In truth, given the enduring strength of Irish nationalism and the weakening of British power caused by the European War, one may well doubt if any measures taken by England—repressive or conciliatory—could have averted a separation such as eventually came in 1922 (an event from which Birrell derived a wry satisfaction: at least he had had the wisdom to resist the folly of attempting to impose conscription upon Ireland). Birrell's greatest blindness lay in his thinking that Easter Week had not been a real rebellion, but only a riot on the order of Sydney Street.

The nature of the dilemma that Birrell faced is well analyzed by Dr. Ó Broin in this sympathetic and fair-minded study. Fifty years after the Rising it is possible to do justice not only to Redmond and Dillon, but to Birrell as well. He was in many ways one of the best chief secretaries Ireland ever had: he took a genuine interest in the Irish Literary Renaissance, was a warm supporter of the Abbey Theatre and secured for Yeats a pension from the Civil List. He came to love Ireland, and in his last days still cherished memories of Connemara and of Kerry. He was especially proud of his part in creating the National University in 1908, and gratified to receive, not long before his death, an honorary doctorate from that institution.

Drawing largely on manuscript sources—among them the Birrell, Nathan, and Asquith Papers in the Bodleian, the Cabinet Papers in the Public Record Office, and the Redmond Papers in Dublin—Ó Broin has written a lively, perceptive, and well-documented narrative that admirably complements his earlier work on Birrell's under-secretary, Sir Matthew Nathan.

University of Washington

GIOVANNI COSTIGAN

THE IRISH CONVENTION, 1917-18. By *R. B. McDowell*. [Studies in Irish History, Second Series, Volume VI.] (Toronto: University of Toronto Press. 1970. Pp. viii, 240. \$8.50.)

EVEN the archenemies of the Whig interpretation may wonder if the Irish Convention, which failed and which, as some historians believe, was not intended or expected to succeed, deserves 220 pages. This book, at any rate, does not convince me that the convention was anything but "a rhetorical irrelevance on the margin of Irish history."

McDowell struggles to make a case for the convention (and his book) by writing in the conditional. *If* this gathering called by Lloyd George as a result of the Easter uprising and its repression, in which every Irish interest, "estate," and opinion was to be represented (but which was boycotted by Sinn Féin) *had not* been prevented by the intransigence of the Ulster Unionist delegates from fulfilling its "assignment" of devising a home-rule scheme for all of Ireland, *would whatever* scheme that *might have been* agreed upon *have been* acceptable to the demos of the twenty-six counties dominated by Sinn Féin? McDowell thinks it might have been. But since *in fact* the Ulster Unionists gave "not an inch" and since *in fact* no scheme could be agreed upon, why bother even to ask the question?

Enraptured, if not enslaved, by the documents of various conventioners and deluded by his own will-to-believe in the power of conferences, McDowell has produced an account so burdened with irrelevance that the reader does not get a clear idea of why the convention met, or of its non-doings, or of why it failed.

But why it failed is not the big question. Did Lloyd George even intend or expect

it to succeed? A. J. P. Taylor and Maureen Wall have, independently, given cogent—and brief—arguments that he did not. The convention was a pseudo-event, a committee to do no business. McDowell appears to be entirely ignorant of those arguments and, as a result, has not examined his materials in the light of them. Alas!

York University

JOSEPH M. WOODS

CALVINISM AND THE AMYRAUT HERESY: PROTESTANT SCHOLASTICISM AND HUMANISM IN SEVENTEENTH-CENTURY FRANCE. By *Brian G. Armstrong*. (Madison: University of Wisconsin Press. 1969. Pp. xx, 330. \$12.50.)

IN seventeenth-century France, Calvinism sustained an internal crisis, which has received relatively little attention from historians. The drama revolved around opposing views of Calvin's doctrine of predestination. Two groups confronted one another: one was inspired by the humanistic tradition that had produced Calvin himself, and argued that justification by faith was the central dogma of his theology; the other, more scholastic in orientation and inspired more by Beza than by Calvin, saw predestination as the defining element of Protestantism. Since the latter group had already established the support of the Synods as well as of custom by 1600, it became known as "orthodox," while the former ultimately went on trial for heresy. Moïse Amyraut (1596–1664) was the chief spokesman for the "heretics." As pastor and teacher, he dominated the Reformed Academy of Saumur for thirty-eight years and published widely on questions of religious dogma, morality, history, and controversy. His work was both critical and constructive: he charged that the Reformed Church had drifted away from Calvin's principles by stressing a harsh concept of predestination; and, citing Calvin extensively, he argued for the universality of redemption and the primacy of faith.

A new book on Amyraut needs no justification, especially when it is as thorough and as impressive as Armstrong's. The first section of the book describes the historical and theological background to the debate and Amyraut's life and works. The major part analyzes Amyraut's rift with orthodoxy, the doctrine of predestination, Amyraut's concept of it, and his insistence upon "faith."

Three hypotheses emerge. First, Armstrong contends that Amyraut was substantially correct in his claim of faithfulness to Calvin's original intent and spirit. Armstrong draws two conclusions from this: that the Reformed doctrine expounded by the Synods was a distortion of Calvin's doctrine, and that undue emphasis has been placed on "predestination" as the pivotal concept of Calvin's faith. Secondly, he contends that Amyraut's theology has not been properly understood. In a series of complex arguments, he demonstrates, as no other researcher has, the centrality of a historical theory of the Covenant of Grace to Amyraut's theology, and that Amyraut's methodology is the distinguishing characteristic of his system. Thirdly, Armstrong takes issue with those who have claimed that Amyraut was a rationalist, and shows that, far from being wedded to logic, he had a "dynamic, existential understanding of the faith."

From the historian's viewpoint, Armstrong's final chapter, though a useful recapitulation, leaves some important threads untied. For example, from his preface one might have expected a short commentary on the links between Amyraut and the coming Enlightenment, but this is not given. Further, one is not told how Amyraut's humanism ultimately affected the scholasticism of his orthodox colleagues. Indeed it is commonly held that Amyraut's school was the exception and not the rule, and yet Armstrong's study, with little supporting evidence, leads one to think that Saumur

dominated seventeenth-century French Calvinism. But the book's strengths outweigh these weaknesses. The whole of the text is marked by clarity of exposition, objectivity, and penetrating insight. To have maintained these qualities throughout the analysis of such a complex subject is more than commendable.

State University of New York, Buffalo

ELISABETH M. ISRAELS

GABRIEL NAUDÉ, 1600–1653. By *Jack A. Clarke*. (Hamden, Conn.: Archon Books. 1970. Pp. 183. \$7.50.)

LES ALMANACHS POPULAIRES AUX XVII^e ET XVIII^e SIÈCLES: ESSAI D'HISTOIRE SOCIALE. By *Geneviève Bollème*. [École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Livre et société: Études et mémoires pour servir à l'histoire de la civilisation du livre, Number 3.] (Paris: Mouton & Co. 1969. Pp. 147. 22 fr.)

NOUVELLES ÉTUDES LYONNAISES. By *R. Chartier et al.* [Centre de Recherches d'Histoire et de Philologie de la IV^e Section de l'École Pratique des Hautes Études. Series VI, Histoire et civilisation du livre, Number 2.] (Geneva: Librairie Droz. 1969. Pp. xiv, 250.)

ONE of the most persistent trends in contemporary historical literature is to examine events and lives by the method of sociological analysis. This approach has begun to influence the writing of intellectual and cultural history. The three works under consideration are good examples of this tendency and therefore should bring to the reader the possibility of greater historical understanding.

Jack A. Clarke, who has earned degrees in history and library science, has utilized his unusual training to produce an interesting and scholarly biography of Gabriel Naudé, who created the Mazarin Library. In emphasizing Naudé's role as a librarian, Clarke has presented more than just the picture of a scholarly and dedicated bibliophile. Unlike previous works in English about the organizer of the Mazarin Library, this study integrates Naudé's career with its social and intellectual milieu. Clarke points out the close relationships that existed among families of the Robe, owners of private libraries, and scholars. The author also clearly indicates that by recognizing the need to open private libraries to public use, Naudé helped to establish one of the bases for modern French scholarship.

What should be interesting and new to most American readers is the account of Naudé's methods in organizing the Mazarin Library and his unsuccessful attempt to save his creation from dissolution during the *Fronde*. Naudé emerges from the pages of this little book as a tragic figure and as the archetype of the perfect seventeenth-century librarian—a cosmopolitan scholar dedicated to the collection, organization, and care of books.

Geneviève Bollème's work is a continuation of her investigations into popular culture and has been influenced by the work of Robert Mandrou. In this long essay, *Almanachs populaires aux XVII^e et XVIII^e siècles*, Bollème has admirably introduced her reader to a literary form seldom dealt with in the cultural history of the period. The history of the popular almanac, according to the Miss Bollème, is the history of the society that produced it.

Two themes pervade the work—the evolution of the popular almanac as a genre for the non- or semi-literate *masses populaires* and, closely connected with that theme,

the almanac as a source of popular wisdom and education existing parallel to, but not generally influenced by the literature of the Lumières. Relying heavily on a limited number of original sources. Bollème analyzes the role of the almanac as a guide to daily life and moral behavior, as well as a source of historical information to satisfy the popular desire for the fantastic and for the factual. The work is often heavy reading and a little repetitious. This is a minor point, however, considering the contribution the essay makes to a better understanding of pre-revolutionary popular culture.

The third work under consideration, *Nouvelles Études Lyonnaises* is a companion piece to the earlier *Cinq Études Lyonnaises* (1966), and is also under the editorship of H. J. Martin. The *Nouvelles études* is divided into two parts. Part I contains four rather specialized essays on the publishing industry in Lyon during the seventeenth century. Part II is a long essay by Roger Chartier, "*L'Académie de Lyon au XVIII^e siècle, 1700-1793, Études de Sociologie Culturelle.*" Anyone familiar with recent trends in the literature on the history of Lyon should not be surprised by any of Chartier's conclusions. What is of merit, however, is his use of statistical analysis to clarify the *haut bourgeois* character of the academy and the dissimilarity between it and the real interests of the urban population of Lyon. Chartier's conclusion that the academy was less an institution of local scholarship than a symbol of bourgeois solidarity is borne out by his analysis of the composition, interests, and influence of the academy.

Vanderbilt University

PAUL DOBSON

LA POPULATION DE MEULAN DU XVII^e AU XIX^e SIÈCLE (VERS 1600-1870): ÉTUDE DE DÉMOGRAPHIE HISTORIQUE. By Marcel Lachiver. Preface by Pierre Goubert. [École Pratique des Hautes Études—VI^e Section. Centre de Recherches Historiques. Démographie et sociétés, Number 13.] (Paris: S.E.V.P.E.N. 1969. Pp. 339.)

AZEREIX: LA VIE D'UNE COMMUNAUTÉ RURALE À LA FIN DU XVIII^e SIÈCLE. By Annie Zink. [École Pratique des Hautes Études—VI^e Section. Centre de Recherches Historiques. Démographie et sociétés, Number 11.] (Paris: S.E.V.P.E.N. 1969. Pp. 323. 44 fr.)

MEULAN is on the Seine, downstream and about twenty-five miles from Paris. Although small with 1,782 inhabitants in 1816, it was otherwise urban, being an important crossroad, a market for wheat from the Vexin and for wine from the valley, and an industrial site with tanneries and, from the middle of the eighteenth century, with cotton stocking frames. The town has unusually full demographic evidence. This enabled Lachiver to perform the unprecedented feat of reconstituting 1,430 families, discovering the terminal date of the marriage for 1,231 and the exact date of the wife's birth for 1,188 of them. The dates of the marriages analyzed are well distributed over a long period, 1660-1839.

The abundance and precision of Lachiver's information is matched by the excellence of his analysis, presented in some seventy pages of text with innumerable tables and graphs, followed by thirteen tables of data. He made some notable discoveries. Regularly after 1714 about two-thirds of Meulan's newly married men were migrants from elsewhere, increasingly from outside the nearby region and from increasing distances. Some women took in nurslings, but many others sent their infants out to

wet nurses in nearby villages, a factor contributing to the brief average interval (22 months or less) between births. Soon after 1740, women over thirty began to bear consistently fewer children. Just after 1790, birth rates fell among all age groups and social classes.

Lachiver shows that limitation of births must have involved an increasing effect of deliberate choice. He suggests that this was made possible by "dechristianization"—a decline in religious fervor or a separation of conduct from belief, or both. Clearly, relations between men and women were changing in several ways during the late eighteenth and early nineteenth centuries. It was more common for young women to possess at least minimal literacy (the proportion rose from sixty per cent in the 1760's to eighty per cent after 1800), to migrate a long distance to Meulan, to become pregnant while unmarried, and to secure marriage after becoming pregnant. These changes occurred simultaneously, perhaps coincidentally, but each marked some further departure from the passivity of women in earlier decades. The protected situation of women was also no longer as well maintained as formerly, if we may judge by the increase in the number of unmarried mothers. Lachiver's admirable work thus provokes new questions, some of which might be answered with data from Meulan.

Azereix is a village about twelve miles from the northernmost Pyrenees, at the edge of the plain through which the Adour river flows and five miles southwest of Tarbes. In her book, Miss Zink tries to elucidate the relation between population growth there and economic and social change, making comparisons with eight neighboring parishes. The attempt is ambitious, laudable, and conscientious, but not altogether successful.

The demographic analysis is based mainly on parish registers for a short period, and on the reconstitution of 265 families, of which 180 were marriages terminated by the death of one of the partners during the wife's childbearing years. On the average, in Azereix, ten families would have a total of thirty-seven children. The comparable total in Meulan during the half-century before 1790 would be forty-eight children. The excess of births over deaths in Azereix would indicate a rapid population growth there from 1755 through 1773, then sporadic upward movement. From 1770 to 1790 the number of *feux* increased from 140 to 171, and there was emigration that cannot be measured precisely. The premise that population growth was pressing against economic resources is not absolutely secure.

The economic description, the longest part of the book, concentrates on agriculture and is largely based on land-use surveys, especially a *terrier* completed in 1767 and a classification made for the *cadastre* in 1818. Many local particularities are discussed in detail, and the uncertainties arising from the evidence are examined with care, but the broad lines of change do not emerge very clearly. The third part of the book is a sketch of class structure, community institutions and politics, and family relations. In her conclusion, Miss Zink suggests that the introduction of maize from 1730 onward resulted in better nutrition and thus in population growth during two decades after 1753. This in turn led to clearings, an increase in the area of ploughed land and meadows, and an increase in the number of livestock. But otherwise there were no drastic changes. The larger number of landless and almost landless agricultural workmen was still subordinate to a nearly autonomous oligarchy of landed villagers whose dominance was secured by the Revolution. The analysis is thoughtful, but the effort to disentangle the causes and consequences of population growth is not finally persuasive. As Miss Zink herself says, the region was rich and expanding. Her

book illustrates the difficulty of clarifying the complex interactions that occurred in such circumstances.

Stanford University

PHILIP DAWSON

CARDINAL DE RETZ: THE ANATOMY OF A CONSPIRATOR. By J. H. M. Salmon. ([New York:] Macmillan Company. 1970. Pp. 447. \$8.95.)

FROM Retz's own brilliant memoirs and letters, as well as from those of other literary *frondeurs*, Professor Salmon has written a succinct narrative of the famous rebel cardinal's life. Neither manuscript sources nor the general works on the Fronde and Jansenism, to mention only two examples, are much relied on to verify the memorial accounts. There are remarks about Retz's search for a personal identity and conclusions about the many roles the cardinal played or tried to play, but these do not add up to a convincing analysis of his personality or historical significance. Nor is this a contribution to the general knowledge of the social history of aristocratic churchmen, which is particularly regrettable because Retz's income, expenses, indebtedness, and will provide such rich source material for the subject.

Yet to ask any historian to supersede Retz's own portrait of himself would be too demanding. The cardinal seems to have been extremely candid about his love life and his activity as a plotter in the Fronde and as a negotiator at various conclaves in Rome. But dare one take Retz at his word, as Professor Salmon so often does? By not methodically comparing the memoirs with other accounts of the same incidents in his life, and with the correspondence of both Retz's friends and his enemies, Professor Salmon may have missed an opportunity to reach a deeper understanding of the man and his times. Is there a layer of conscious or unconscious rationalization or distortion in Retz's memoirs? This question would seem to be elementary for historical biography, but it is never posed.

Retz's lust for power, his desire to be treated as a *grand homme*, even though the Gondi family lacked this status, his nearly complete freedom for conventional religious and political morality, and his inability to come to terms with Mazarin all are touched on here but are never thoroughly elucidated. Nor is Professor Salmon always quite fair. To depict Mazarin as obsessed with Retz's ruin distorts the historical assessment of the former. When Retz refused to give up and kept on publishing attacks on Mazarin and his policies, it is not surprising that Mazarin refused to make concessions. An impartial perspective is almost unconsciously lost when Mazarin's difficulties, which also included exile and threats of disgrace and assassination, are brushed off as "set backs," while for Retz these things were "painful" and evoke the author's sympathy.

The Johns Hopkins University

OREST RANUM

CAPITALISTES ET POUVOIR AU SIÈCLE DES LUMIÈRES: LES FONDATEURS DES ORIGINES À 1715. By Claude-Frédéric Lévy. (The Hague: Mouton. 1969. Pp. viii, 502. 32 gls.)

NEITHER the title, the chapter headings, nor the section titles of this work tell us what to expect. After reading it—with much irritation but also with a good deal of interest—I am still not sure of the author's purpose. Nor am I certain for what sort of audience he intends this work. Much of it deals with a large number of French and Swiss merchants and financiers who were in some way involved in supplying and

financing the wars of Louis XIV and, particularly, the War of the Spanish Succession. Lévy's method might be described as pointillist—bits of personal, family, business, and local history against background sketches of general political history, making for a bewildering canvas. Section III, "The Gate-Keepers of the West," gives valuable details of business history, especially concerning how capitalists strove to profit from France's incursions into the slave trade with Spanish America. Other sections with equally enigmatic titles provide insights on the failure of the government to solve the eternal problem of how to move large volumes of specie to pay its troops fighting abroad. Lévy shows how this failure presented golden opportunities to Swiss bankers and their French associates. But Section II, "The Catacombs," is an astonishing digression on the significance of a secret marking—two slashes and two dots—found placed next to many of the signatures on the documents Lévy studied. And there are several chapters dealing with military affairs and court intrigue that seem strangely out of place. There are no conclusions, no explanations of what Lévy considers to be his contributions in this work. In fact there is no analytical apparatus at all. Each of us is allowed to take what he wants from this overlaid table.

University of Pennsylvania

MARTIN WOLFE

CLAUDE BAUDARD DE SAINTE-JAMES: TRÉSORIER GÉNÉRAL DE LA MARINE ET BRASSEUR D'AFFAIRES (1738-1787). By *Denise Ozanam*. [Musée du Fer, Number 1.] (Geneva: Librairie Droz. 1969. Pp. 214.)

THIS thesis of the *troisième cycle* is a logically structured, carefully written, and superbly researched study of the relationship between high finance and the royal administration of eighteenth-century France. Based largely on manuscript sources from twelve different repositories, it is a good example of what an imaginative historian can do despite the lack of a subject's personal papers. It should be read by all students of the period who use French notarial records.

The author has collected a massive amount of evidence to prove that Baudard was "a man with many irons in the fire." More than half of the book is devoted to a detailed description of Baudard's financial investments: land and houses, manufactories of sailcloth and armaments, mines and annuities, a discount bank and a commercial company, and a vast plan to furnish Paris with water. The last fifty pages deal with an overview of Baudard's financial affairs and an analysis of the reasons for his declaration of bankruptcy in February 1787. In some ways the book is disappointing. Baudard appears as a shadow figure who invested heavily in a multitude of enterprises, many of which provided materials needed by the navy, and then discovered that he had built a financial house of cards. The explicit reasons for his bankruptcy are not clearly explained; nor is there sufficient evidence to support the author's claim that Baudard was financially innovative and politically powerful.

Still, the book is important because it goes beyond Baudard and suggests another case of the French monarchy resorting to expedients in its effort to resolve its vexing financial problems. Because the monarchy was unable to secure adequate capital from conservative bankers and large merchants, it selected wealthy financiers like Baudard as treasurers of the navy and war departments. The government expected these individuals to find the funds necessary for its most urgent needs and, in return, it closed its eyes to conflicts of interest and to the treasurers' use of departmental funds for private investments. The expedient might have worked, the author argues, if Baudard and other officers-financiers had not been bound by archaic laws governing

financial enterprises, limited by undeveloped credit facilities, and caught up in the general economic crisis of the 1780's.

State University of New York, Binghamton

THADD E. HALL

REPRÄSENTATION UND REVOLUTION: EINE UNTERSUCHUNG ZUR GENESIS DER KONTINENTALEN THEORIE UND PRAXIS PARLAMEN-
TARISCHER REPRÄSENTATION AUS DER HERRSCHAFTSPRAXIS DES
ANCIEN RÉGIME IN FRANKREICH (1760-1789). By *Eberhard Schmitt*.
[Münchener Studien zur Politik, Number 10.] (Munich: Verlag C. H. Beck. Pp. xi,
334. DM 46.)

LE CONCEPT DE REPRÉSENTATION POLITIQUE AU DIX-HUITIÈME
SIÈCLE FRANÇAIS. By *Jean Roels*. Preface by *Marcel Prélot*. [Anciens pays et
assemblées d'états: Études publiées par la Section belge de la Commission Interna-
tionale pour l'Histoire de Assemblées d'États (Centre National de Recherches,
A.S.B.L.) avec le concours du Ministère de l'Éducation nationale et de la Culture,
ainsi que des Gouvernements des Provinces de Brabant, de Hainaut, de Liège, de
Luxembourg et de Namur, Number 45.] (Louvain: Éditions Nauwelaerts. 1969.
Pp. xvi, 184. 490 fr. B.)

BOTH books are about the intense controversy over political representation that occurred in France at the end of the old regime. Primary attention is given to the side that broke with tradition and eventually triumphed in the National Assembly, but Schmitt also considers the revival of interest in government by estates. It is an important subject to which the authors contribute insight and useful information, but neither study can be taken as a comprehensive or decisive treatment. In fact, to read them through is something of a chore, though they may be consulted with profit. Schmitt has labored with immense energy; his bibliography of published and unpublished materials is impressive, and his prodigious footnotes are overwhelming. Moreover, the last part of the book, dealing with pamphlet literature in 1788-89 and with day-to-day maneuvers that transformed the Estates General into a national assembly, is most rewarding. Unfortunately, before reaching these passages the reader must exhaust himself with theoretical discussions of representation and revolution. The first is never clearly related to the narrative, and the second, though relevant, is not particularly interesting. There are also descriptions of familiar events like the revolt of nobles in 1787-88, which are part of the narrative but add little to our knowledge. Perhaps the author, who relies heavily on works by French, English, and American historians, is merely bringing his German audience up to date. On the other hand, Schmitt fails to develop his stimulating assertion that concern for the Estates General was the central political theme of the second half of the eighteenth century. We really need a balanced interpretation of the part played by aristocracy in the modernization of France.

Roels' book consists of two loosely connected essays on the problem of representation as formulated by Rousseau and Emmanuel Sieyès. A straightforward exposition of the relevant texts is provided with scarcely any biographic detail. The chapters on Rousseau are sensible but do not, in my opinion, reveal anything an educated reader could not find for himself. Indeed, North Americans caught in a crisis of abundance might give to Rousseau's dislike of economic growth the attention it deserves. The author finds Rousseau not so rigidly opposed to representation as some statements

in the *Social Contract* suggest, but opposition remains the dominant note. This is not surprising, given the corruption of parliamentary institutions in the eighteenth century. Rousseau's suspicion is also justified after the fact by the example of Sieyès, whose theory of representation rejected aristocratic privilege without coming any closer to the popular democracy cherished by the Genevan. Above all, Roels accepts a degree of impracticability in the *Social Contract* because he thinks its purpose, and certainly its effect, was less to prescribe action than to offer each succeeding generation a timeless standard of freedom and morality. The section on Sieyès is more interesting in the sense that it illuminates a personage not so well established in history. The abbé, also greatly admired by Schmitt, emerges from this study as an original thinker whose great contribution was to explain how a legislative body placed at the center of government could first create the nation and then become its chief representative. Sieyès is equally fascinating in his attempt to associate the National Assembly with new economic forces in Europe. "The political systems of today are founded exclusively on work," Roels quotes him as saying. We are reminded how useful it would be to have an inexpensive English edition of Sieyès' writings for our undergraduate students.

University of British Columbia

DANIEL M. KLANG

LOUIS-PHILIPPE DE SÉGUR: AN INTELLECTUAL IN A REVOLUTIONARY AGE. By *Leon Apt*. [International Archives of the History of Ideas, Number 25.] (The Hague: Martinus Nijhoff. 1969. Pp. xv, 161. 28.80 gls.)

TALLEYRAND, STATESMAN PRIEST: THE AGENT-GENERAL OF THE CLERGY AND THE CHURCH OF FRANCE AT THE END OF THE OLD REGIME. By *Louis S. Greenbaum*. (Washington, D. C.: Catholic University of America Press. 1970. Pp. vii, 293. \$12.50.)

Louis Philippe de Ségur (1753-1830) is probably less well known to historians than his father Philippe Henri, a celebrated soldier who became minister of war under Louis XVI, or his son Philippe Paul, a Napoleonic general and memoir writer. Leon Apt's succinct survey of the middle Ségur's life and extensive writings is an appropriate introduction to a significant body of material and will henceforth be an essential point of departure for those who want more. Ségur had almost as many lives as Talleyrand, whose conduct in the office of agent general of the clergy of France is the occasion for Louis S. Greenbaum's revealing study. One would have thought that Talleyrand needed no further introduction, but Greenbaum has provided one that, henceforth, biographers or students of old regime institutional history will be unwise to overlook.

Ségur and Talleyrand were not alike in character, talents, or effectiveness, but they were both court nobles, born a year apart, who were ambitious and worked within the system prior to the Revolution. Ségur, who had better connections, early became an army officer, helped his father at the war ministry, went on an authorized mission to America (he had refrained from going unauthorized with his friend Lafayette), and served as ambassador to Russia from 1785 to 1789. Apt has more details about this early period than about any other, for it is the subject of Ségur's memoirs, which go to 1789, and of his history of international and French affairs from 1786 to 1796, the two publications Apt finds most valuable. Apt has fewer biographical details from the revolutionary years, when Ségur's Anglophile opinions and his

ambitions for a ministerial or diplomatic post were soon left behind by events. There was a moment in 1791 when both Ségur and Talleyrand were being considered for the mission to England that subsequently enabled Talleyrand to emigrate between 1792 and 1796. Ségur lost his fortune but lived through the Terror without emigrating and after Thermidor became a successful journalist and historian, amply justifying Apt's subtitle. By 1795 he was making Sieyès-like efforts to reconcile representative government with containment of the masses, and one could wish that Apt had turned up more details about this period; what he reports seems to anticipate the Bonapartist that Ségur was to become, but in the few pages devoted to the Directory period Apt does not describe Ségur's movement away from the royalists and moderate republicans who were the heirs to his old Anglophile preferences.

Brevity, precision, and fair-mindedness combined with a good inventory of Ségur's life and work characterize this book, but one cannot help wishing that the author had allowed himself more space for discussing the intellectual history he so obviously relishes; he is a persistent searcher for philosophy of history and for signs of romanticism, but it is not always clear what other questions he was asking in relation to the Ségur materials. He does, to be sure, bring forth tantalizing perspectives from Ségur's many-volumed universal and French histories, and on the basis of the political essays and speeches of the Restoration period (Ségur was a Peer), he makes plain that his subject had achieved a mature form of early liberalism akin to that of Constant if not as systematic. One is left in some uncertainty as to Ségur's depth, but there is no doubt about his significance as a representative of his class (though he was not typical) and of his time.

The greater weight of Talleyrand's career and the amount of attention it has received make Louis S. Greenbaum's specialized ground-breaking study especially appropriate. Talleyrand's young uncle, himself on the way up the ladder of influence in the Church, secured the general agency for him well in advance and guided his steps toward it; Talleyrand went into it with his eyes open, thoroughly trained at the Seminary of Saint-Sulpice and at the Sorbonne; from 1780 to 1785, between the ages of twenty-six and thirty-one, he served with indubitable distinction, self-confidence, authority, executive ability, industry, and conservatism.

Talleyrand, whose Richelieu-like qualities Greenbaum leaves in no doubt, is, however, only one of the protagonists of this history; the other is the general agency itself, or rather, the whole structure of Church administration and its dynamic relationships with various levels of the clergy and various agencies of the state as well as with tithes, *décimes*, judicial functions, the management of properties, the advancement of personnel, and the flow of information to and from the 40,000 parishes of France. For those who relish the exercise of judgment and the complexity of institutions and the reconstruction of these past glories from rich archives such as those of the French Church—the minutes of its legal staff, the correspondence of its agents general, and the records of its general assemblies, for example—the story of Talleyrand's superb competence exercised in a conservative cause will be impressive, and so will Greenbaum's formidable bibliography and extensive notes. Talleyrand, he writes, was by 1789 “already planning an opportune exit.” The competence was to be shifted to the service of the state. This book is both a manual of old regime lore and an important biographical essay.

Swarthmore College

PAUL H. BEIK

JEWES AND THE FRENCH REVOLUTIONS OF 1789, 1830 AND 1848. By *Zosa Szajkowski*. (New York: Ktav Publishing House. 1970. Pp. lv, 1161. \$29.50.)

THIS is an imposing work—well over a thousand pages in length, containing reprints of more than forty articles and other writings by the author, published over a period of twenty-five years. Moreover, it is, as Szajkowski indicates in his introduction, eminently “serious” scholarship, backed up with an almost overwhelming force of footnotes and a periodically intimidating reference to the authority of “the documents,” some of which are in the author’s possession. Its appeal, therefore, is likely to be limited to specialists—others will be put off by the grim presentation, by the heaviness of the prose, by the somewhat abstruse subject matter of many of the articles, and by the author’s inability to relate his material to other, more general themes of either French or Jewish history. But specialists too will have difficulty. The long and rambling introduction is marred by a number of misprints and grammatical errors; one of the articles is cut off abruptly before the end; there is a great deal of repetition; and the articles do not seem to have been selected in any purposeful way. Inexcusably for a book of this character, there is no index. Even more serious difficulties arise with the author’s historical method. The narrative invariably lacks a sense of scale; the reader is taken through a dense forest of detail and is only occasionally wrenched into the freer but extremely thin air of generalization. This makes for a bewildering and boring journey, for me at least. And finally, having gone through over a thousand pages of political, social, and economic history of French Jews, the reader will be left with practically no conception of what it was actually like to be Jewish in France between 1789 and 1848. He will emerge with a greater understanding of the many divisions within French Jewry, with a knowledge of statistics on all sorts of demographic and other matters but with little sense of what it all means.

Despite these difficulties, however, this is still a useful collection, and an indispensable starting point for anyone wishing to do further work in Jewish history in France during the period. For Szajkowski has worked with enormous industry and thoroughness. He has examined synagogue records, departmental archives, provincial newspapers, pamphlet collections, local histories, and a host of other sources. He has drawn together an extraordinary amount of information, much of which is important, and most of which is not available elsewhere. The book is, then, a place to begin. If its conclusions and analyses are somewhat inadequate, its indication of the diverse materials by which the Jews can be studied is a considerable contribution.

University of Toronto

MICHAEL R. MARRUS

MADAME ROLAND AND THE AGE OF REVOLUTION. By *Gita May*. (New York: Columbia University Press. 1970. Pp. x, 360. \$10.00.)

MME. Roland, wife of an inspector of manufactures of the old regime and minister of the interior of the First Republic, suffered the fate of many prominent figures in the French Revolution. She not only fell victim to the Terror in 1793 but ever since has been the subject of sharp controversy between her romantic admirers and her denunciators from Left and Right. This biography, based in large part on Mme. Roland’s own writings and other contemporary literary sources, is a scholarly and, on the whole, successful effort to provide a more credible and balanced view.

Mrs. May excels as a biographer. She is so thoroughly acquainted with the literature of the eighteenth century and with the milieu of her subject that she can penetrate

deeply the mind of this intellectually precocious woman. She is able to reveal not only the character and ideas of Mme. Roland but to throw them into relief against the intellectual climate of her generation. An admirable tone of objectivity prevails throughout. Although Mrs. May has obvious sympathy for the courage and intelligence of her subject and admires the integrity of her character, she has no difficulty in recognizing the priggish smugness, naivete, and subjectivity that marred her judgment of men and events.

Specialists in the French Revolution will find Mrs. May less successful in coping with the historical framework surrounding her subject. An authority on eighteenth-century French literature, she is on much less familiar ground in political and economic history. Mrs. May makes no great effort to determine precisely the degree to which Mme. Roland influenced her husband in his political life. The study by C. A. Le Guin, *Roland de la Platière. A Public Servant in the Eighteenth Century* (1966) could have helped her in that question. Mrs. May relies entirely on secondary accounts for the events of the Revolution and their interpretation. Although she has used works of historians representing many different schools of thought (Albert Mathiez, Kingsley Martin, Peter Gay, Ida Tarbell, Albert Soboul, Hippolyte Taine, to name a few contrasting personages), she sometimes seems unaware of the bias or issues at play in their views. She is acquainted with M. J. Sydenham's *The Girondins* (1961) yet still tends to present the so-called Girondins as a bloc or party. Perhaps she absorbed too much of her subject's detestation of Robespierre. Attributing to him more malice aforethought and personal power than he ever possessed, Mrs. May views him throughout, explicitly or implicitly, as the moving spirit of the Terror and the sole dictator of the republic of virtue.

The book is eminently readable. Although slow at first—the daily round of an engraver's daughter is not the stuff of which great drama is made—it gathers speed and broadens in scope with the coming of the Revolution until the last turn into tragedy. Mrs. May depicts in moving terms the bewilderment and despair of one who had greeted the outbreak of revolution with passionate joy only to find that it unleashed forces beyond control.

University of Texas, Austin

NANCY NICHOLS BARKER

JACOBIN LEGACY: THE DEMOCRATIC MOVEMENT UNDER THE DIRECTORY. By *Isser Woloch*. ([Princeton, N. J.:] Princeton University Press. 1970. Pp. xi, 455. \$15.00.)

THIS book is our first history of national popular politics under the Directory. As such, it contributes significantly to our knowledge and understanding of the period. Using the contemporary press and a mass of archival materials (mainly though not entirely Parisian), Woloch describes the reorientation of Jacobinism after Thermidor, its resurgence in the "constitutional circles" that the Directory tolerated after the coup d'état of Fructidor (September 1797), its creeds, its social composition, its achievements in the elections of 1798 and 1799, its containment by the so-called Second Directory, and its defeat by the coup d'état of Brumaire. Although lacking the membership lists and registers of deliberations used by historians of Jacobinism before Thermidor, Woloch has exploited his materials with resourcefulness and ingenuity and penetrated a milieu that until now we have known only at a distance. His research does him credit.

Fragmentary evidence on the social recruitment of this movement suggests that from a third to a half of its members were artisans and shopkeepers and that the others were

professional men—soldiers, veterans, merchants, cultivators, and officials. Apparently the sources preclude reliable estimates of Jacobin enrollment, but the membership of the twelve Parisian clubs could not have exceeded two per cent of the qualified electorate, and for that reason the success of Jacobin candidates in 1798 and 1799 seems remarkable, even taking into account that only fifteen to thirty per cent of the eligible voters attended the primary assemblies. Apparently, Jacobinism had an influence that far outranged its membership, a fact that underscores how little we know about political opinion as a whole during the Directory.

What is striking about this movement is the moderation of its goals and values. Accepting the Constitution of 1795 as an improvable basis for public life, these Jacobins acted as a loyal opposition that stood for probity and active political participation (*civisme*), vigilance against royalists, vigorous support for the armies thrown back by the Second Coalition, and a modest amount of economic regulation and graduated taxation as the proper approach to social problems. Although they revered Babeuf as a martyr, they ignored his socialism and left it for later generations to discover. This being true, it is hard to understand why the Directorials should have harassed the movement, limited its activities, disqualified so many of the candidates it elected by diligence and effective organization in 1798, and misrepresented its intentions to the public at large. On this problem Woloch has interesting things to say, but they are not entirely satisfying. Although he understands the Jacobins, who command his sympathy, he does not fully explain why the Directorials should have feared them as they did. Readers will be tempted to pursue the matter further for themselves. On the whole, one has the feeling that this movement, in spite of its electoral achievements, was not very robust. It was held in check by the indecisive dictatorship of the Second Directory and became dangerous only when the regime and its supporters were split. The suppression under Bonaparte, which logically completes Woloch's story, is barely described in these pages, but it seems to have succeeded with ease.

In summary, this is a book that everyone interested in the Revolution and the history of French democracy will want to read. It illuminates the politics of a period that remains, in spite of all that has been written about it, poorly known. Its greatest service, however, is to indicate how much we have yet to learn.

University of North Carolina, Chapel Hill

GEORGE V. TAYLOR

LA PREMIÈRE RESTAURATION ET SON BUDGET. By *Michel Bruguère*.

[Centre de Recherches d'Histoire et de Philologie de la IV^e Section de l'École pratique des Hautes Études. Series V, Hautes Études médiévales et modernes, Number 10.] (Geneva: Librairie Droz, 1969. Pp. xxviii, 270.)

THE early years of the Bourbon Restoration were crucial in French history for developing and affirming the institutions of parliamentary government. During the French Revolution and under Napoleon, many ideas in government and public finance had been tried and many more had been discussed, but few had been affirmed. From 1814 to 1819 deputies, peers, ministers, and king, who had lived through the torment of revolutionary experience and who were acting under the imperious necessity of surviving in office, made decisions with respect to electoral laws, parliamentary procedures, and the budget, decisions that became precedents for later generations. Hence, a monograph on the budgets of 1814 and 1815, especially as excellent a monograph as this one is, has considerable historical value. It tells in intricate detail, drawn from manuscript and printed sources, how under the protection of Louis XVIII, the minister of finances,

Baron Louis, formulated the two budgets, proposed them to the deputies and peers, and won approval in open debate. Two factors chiefly affected Baron Louis and his royal master: the example of England and Napoleon's horror of a public debt. The French monarchy of Louis XVI did not really have a budget, and it was difficult for the king himself to discover his fiscal situation. Napoleon had a budget but did not submit it to public debate. Louis XVIII and Baron Louis prepared an annual budget and, following the English model, submitted it to legislative discussion and vote, thus establishing a major precedent. Napoleon's horror of public debt was absorbed by Baron Louis, one of his major treasury officials. In the midst of the fiscal disarray of Napoleon's abdication and Louis XVIII's arrival, Baron Louis insisted on the establishment of sound public credit. He had the royal government assume the debts of the former regime, tried to balance expenses and receipts, and reluctantly continued the hated *droits réunis*. His budgets may have been tactless, but they affected French fiscal policy for decades.

Duke University

HAROLD T. PARKER

VIE COMMERCIALE DE LA ROUTE DU CAP HORN AU XIX^e SIÈCLE: L'ARMEMENT A.-D. BORDES ET FILS. By *Marthe Barbance*. [École Pratique des Hautes Études—VI^e Section. Centre de Recherches Historiques. Ports—routes—trafics, Number 27.] (Paris: S.E.V.P.E.N. 1969. Pp. 372. 49 fr.)

THE first part of this monograph describes the rise of French commerce with the countries of the Eastern Pacific by way of the difficult route around Cape Horn. The second and longer part focuses upon the shipping and trading activities in this area of the firm founded by Antoine-Dominique Bordes. Together they furnish a rather complete history of the role of the sailing ship in French commerce in the Eastern Pacific from its inception in 1819 until the end of World War I.

In the early years the trade consisted mainly of products of great value in relation to their weight: textiles, wines, spirits, and specie. The profitability of the trade was hindered by the lack of a suitable product for the return voyage, but this was remedied by the growing importance of the commerce in tin, copper, guano, saltpeter, and cereals. The demand for capacity to transport these products reversed the imbalance until growing demand for coal for refining and steamships provided an equilibrium. The advent of the steamer forced the sailing ship to concentrate on cargoes of relatively small value for their weight. Depressions and civil disorders greatly affected the profitability and volume of trade. All these matters and many others receive careful attention from the author.

After laying the basis of his fortune by combining the activities of merchant and shipowner in Valparaíso, Antoine-Dominique Bordes returned to Bordeaux in 1868. In the decade following his return, the firm enjoyed the period of its greatest growth, attaining over forty sailing ships. His dealings in commodities were so extensive that he had to resort to the services of other carriers. At one point Bordes tried, unsuccessfully, to corner the world's market in copper. After his death in 1882, the commercial and shipping activities of the firm were simplified, specializing in transporting English coal and Chilean saltpeter, the latter being in heavy demand in Europe as a fertilizer and for explosives. Although the author refrains from making the point explicitly, it is apparent that none of Bordes' three sons possessed their father's drive or entrepreneurial talent.

Until World War I, the sailing ship could still compete successfully with the steamer

for heavy cargoes of relatively small value where speed was not important. During World War I, German submarines took a heavy toll of sailing ships; the Bordes firm lost over half their vessels. After the war the sailing ship was unable to reconquer its position, and the Cape Horn route fell into disuse owing to a variety of causes, including an overabundance of steamship tonnage, the opening of the Panama Canal, a decline in the demand for imported coal, and growing trade restrictions. Mrs. Barbance, using hitherto unexploited archival sources, has made a valuable contribution to the history of commerce.

State University of New York, Binghamton

CHARLES E. FREEDEMAN

BLANQUI. By *Samuel Bernstein*. Translated from the English by *Jean Vaché*. [Bibliothèque socialiste, Number 14.] (Paris: François Maspero. 1970. Pp. 351. 24.65 fr.)

Dr. Bernstein's treatment of the nineteenth-century professional revolutionary is perhaps the best one-volume biography of Blanqui to date. It cannot, of course, encompass the detailed erudition of Maurice Dommanget's many works on the various stages and aspects of Blanqui's career, but it presents a clear and authoritative life based on a mastery of the major primary sources.

This is very much a political biography whose references to Blanqui's personality are usually introduced as partial explanations for aberrations in his political theory and tactics, with little pretense to a full psychological portrait. On the other hand, Blanqui is carefully described as the product of his historical and social milieu—a revolutionary activist who never shook off the experience of the secret societies of the pre-1848 period and a social thinker whose flawed assumptions and limited insights reflected an essentially pre-industrial system where the workshop was more evident than the factory and the artisan than the industrial worker. Bernstein flatly rejects the old canards against the purity of Blanqui's revolutionary commitment. He ranks Blanqui high among his contemporaries for the lucidity and power of his political intelligence, but argues that the conspirator's distaste for theory and the limitations of his background made him unable to grasp the essential direction of economic and social development. This intellectual failure guaranteed the failure of Blanqui's revolutionary adventures. Bernstein deplores the exaggeration of Blanqui's similarity to Marx—where Dommanget sees virtual congruence in such conceptions as the history of class struggle, Bernstein contrasts Blanqui's shallow and anachronistic re-creation of pre-industrial social relationships with the profound and scientific insights of the master. In this case I think Bernstein is closer to the mark than Dommanget, although I do not always agree that where Blanqui deviated from Marx's profound insights he was in the wrong. For example, his skeptical comments on the First International are cited and discussed by Bernstein as a failure of political intelligence, although they did constitute a pretty fair prediction of the actual development and fate of the International.

Dr. Bernstein is clear and unambiguous in stating and presenting his interpretations—an advantage to the extent that the reader always knows where he stands. On the other hand the reader often has to settle for flat assertions that disregard or succinctly dismiss opposing views. Thus Proudhon is unquestionably a reactionary and enemy of progress; Blanqui completely missed the historical point in denigrating Robespierre; and the French "bourgeoisie"—more or less hypostatized—consciously adopts policies appropriate to its historical situation.

One does not have to accept all of Dr. Bernstein's interpretations to grant that they are based on a remarkable grasp of relevant sources and that the book is a worthy addition to his bibliography in the field of French social and political history. It is regrettable that this fine product of American scholarship is not available in English.

University of Iowa

ALAN B. SPITZER

LA II^e RÉPUBLIQUE (1848–1851). By *Louis Girard*. [“Naissance et mort . . .”]
(Paris: Calmann-Lévy. 1968. Pp. 318. 19.90 fr.)

LA SECONDE RÉPUBLIQUE, CHARLES-ALBERT ET L'ITALIE DU NORD
EN 1848. By *Ferdinand Boyer*. [Bibliothèque de la Revue d'histoire diplomatique,
Number 2.] (Paris: Éditions A. Pedone. 1967. Pp. 348.)

PROFESSOR Girard's volume is the second in a series purporting to be an analysis of the “*naissance et mort*” of four French republics. If his contribution is representative, the series will be a disappointment. His study does not address itself directly to the issue of birth and death; the reader can never be certain whether the birth was even a live one or when *rigor mortis* set in. Rather, it is a sound, factual, general study of the Second Republic and is in the tradition of narrative political history. At times brilliant insights emerge in a sentence or two, only to resubmerge, lost in the narrative. They do not add up to a new interpretation. Girard's major contribution is the balance and impartiality of his account of the second experiment in republican government, over which Frenchmen still debate heatedly.

Professor Boyer's monograph is also in the tradition of political and diplomatic history. It is a highly detailed narrative, based on exhaustive research in the French foreign office archives, as well as in those of the French ministry of war, and in those of Turin and Milan. The documentation is impressive, the organization is well carried out—given the need to master and orchestrate a myriad of detail in an almost day by day running account—and the conclusions are sound.

Boyer's aim is to disprove the assertion of several Italians that France treated their home country unfairly during 1848. On the contrary, France was quite willing to aid Piedmont-Sardinia, Lombardy, and Venice against the Austrians. In return for such aid Lamartine was prepared to demand the reversal of the 1815 settlement: if northern Italy was to be unified under Charles-Albert, then France must rearrange her frontiers by the acquisition of Savoy and possibly Nice. Eugène Cavaignac and Jules Bastide, who replaced the poet, were decidedly more generous and offered French armed intervention against Austria without territorial compensation. At first Sardinia's rulers, absolutists all, were too fearful of the republican propaganda that would accompany French troops; besides, they believed that *Italia fera da se*. Not until late August, after the serious defeat of the Italians by Marshal Radatsky, did the call—even yet reluctant—go out for French aid. By this time France had suffered domestic turmoil, she would have to face the victorious Austrian army almost alone, Germany and Russia favored the Hapsburgs, and Great Britain was resolutely hostile to French intervention. In fact, Palmerston's opposition to French aid was a second factor that caused the small Sardinian forces and other Lombard patriots to stand alone before the large imperial army. Professor Boyer's book is convincing, interesting, and definitive. Barring the discovery of new documents it will stand as the major study for many years to come.

State University of New York, Buffalo

LEO A. LOUBÈRE

GEORGES MANDEL: UN CLÉMENCISTE EN GIRONDE. By *B. Favreau*. Preface by *Albert Mabilleau*. [Bibliothèque de l'Institut d'Études Politiques de Bordeaux, Centre d'Étude et de Recherche sur la Vie locale. Series Vie locale, Number 1.] ([Paris:] Pedone. 1969. Pp. vi, 295. 30 fr.)

DESPITE its title, this book is an attempt to give a relatively full account of the career of Georges Mandel, Prime Minister Clemenceau's powerful *chef de cabinet* from 1917 to 1919 and minister of the interior in Paul Reynaud's ill-fated government of 1940. Favreau's study, however, originally undertaken as a thesis for the *diplôme* of the Institut d'Études Politiques de Bordeaux, does give special emphasis to Mandel's political career in the Gironde, the area he represented in Parliament from 1919 to 1924 and again from 1928 to 1940. Utilizing the local press, prefectural reports, and personal interviews, Favreau gives a detailed picture of Mandel's electoral battles, which well illustrate the difficulties that politicians primarily interested in national issues have in getting elected by constituents primarily interested in local affairs. But the section on the Gironde represents only about a third of the book. Of the rest, one section analyzing Mandel's personality and political ideas is useful, but the others, devoted to Mandel's early career and his role as a national politician, are based mainly on the works of Coblenz, Varenne, and Wormser, and contain little new information.

Undoubtedly Favreau is repelled by Mandel's political activities during the 1920's when his penchant for backstage maneuvers combined with his open and insulting denunciations of his colleagues created such animosity that one writer called him "the most hated man in France." Favreau finds this "a detestable period" of Mandel's life. Again, however, Favreau is eventually won over by the Mandel of the 1930's because of his foresighted attempt to get France to resist Nazi Germany by force and his attempt to continue the war from North Africa after the disastrous defeat of the French army in 1940. Favreau concludes that Mandel failed to achieve his aims in foreign and domestic policy largely because he was too much of an individualist to seek or obtain the necessary party support, but he considers Mandel's life to be "an astonishing lesson in courage and perseverance" because he succeeded in overcoming the serious personal handicaps he faced—his Jewish birth, his obscure social origins, his physical frailty and unimposing appearance—to become one of the leading political figures in the last years of the Third Republic.

Queen's University

JOHN SHERWOOD

THE OBSTRUCTED PATH: FRENCH SOCIAL THOUGHT IN THE YEARS OF DESPERATION, 1930-1960. By *H. Stuart Hughes*. (New York: Harper and Row. 1968. Pp. xi, 304. \$6.95.)

The Obstructed Path, a sequel to Hughes's well-known study of the "generation of the 1890's," *Consciousness and Society* (1958), explores the period from 1930 to 1960 in France. Hughes promises a third volume on anti-fascist émigrés from Italy and Central Europe in the same period. The "obstructed path," a metaphor borrowed from Alain-Fournier's novel *The Wanderer*, describes the psychological state of the French intelligentsia who, in the wake of the First World War and the Depression, underwent a crisis of confidence in the worth and direction of their culture. Deliberately bypassing "intellectuals single-mindedly committed to a political cause," Hughes presents five groups—historians (Lucien Febvre and Marc Bloch), Catholics (Gabriel Marcel and Jacques Maritain), writers (Roger Martin du Gard, Georges Bernanos,

Antoine de Saint-Exupéry, André Malraux, Charles de Gaulle), philosophers (Jean-Paul Sartre and Maurice Merleau-Ponty), and a curious trio, Albert Camus, Pierre Teilhard de Chardin, and Claude Lévi-Strauss—in search of a “way out,” a new mode of thought to replace the arrogant self-sufficiency of “classical” French culture.

Like its companion volume, this is an elegantly written and richly suggestive study. The prevailing metaphor is a refreshing organizing principle for the study of major if widely diverse thinkers. The chapter on the rise of the *Annales* group in search of a new kind of history will be of particular interest to readers of this journal. Hughes’s analysis gives rise to some happy juxtapositions: thus de Gaulle joins Malraux and three other novelists as a seeker after heroism in his *Memoirs* and in his life. One of the other combinations is less compelling. Hughes links Camus, Teilhard de Chardin, and Lévi-Strauss, despite their wide divergencies, as heralds of a new French cosmopolitanism in their sources (Algeria, China, Brazil) and their readership. Yet their most obvious point in common, which Hughes fails to mention, is their large audience in America. The reader sometimes has the impression that the way out is across the Atlantic.

Other aspects of Hughes’s interpretation are also open to dispute. The scope, precluding the extended analysis of any one problem, occasionally provokes oversimplification: thus, apropos of Camus’ agony over Algeria, Hughes declares that sympathizing with the Arab revolution “presented no serious moral or political dilemmas” for other intellectuals of the Left (p. 140), ignoring *inter alia* the problem of treason. More broadly, he betrays a distaste for political ideology, which he treats as a kind of weakness, so that Merleau-Ponty’s “ineradicable penchant . . . to look for an immanent logic in the course of human affairs” is his “weakest point” (p. 209). Nonetheless this is a most welcome book. As a rare and imaginative attempt to treat twentieth-century French intellectual life as a whole, it is a remarkable if not always a convincing synthesis.

Yale University

HESTER EISENSTEIN

THE WAR OF SUCCESSION IN SPAIN 1700–15. By Henry Kamen. (Bloomington: Indiana University Press. 1969. Pp. xii, 436. \$8.95.)

KAMEN provides a capsule review of his own book when he writes in the introduction that he has not given a comprehensive survey of Spain during the War of Succession. Rather, he says, he has published the “first analytical work” dealing with the “dark age of modern Spanish historiography.” Kamen is much too modest, for his work is not merely a “first.” Though certainly not a comprehensive and exhaustive view of the early years of Bourbon Spain, it so substantially illuminates many important topics as to force revision of the general treatments of the period.

The author devotes minimal attention to the military and diplomatic aspects of the war, concentrating instead upon the establishment of Bourbon administration in Spain. He gives a thorough analysis of French motivation and methods in commercial and economic penetration, examines the financing of the war, and considers the impact of the struggle upon the social structure of Spain. His chapters on the contest between Bourbon and Hapsburg in Aragon and Valencia are especially informative and, for the first time, show the interesting role of the ambitions and grievances of the peasantry. Moreover, Kamen provides fresh information on such figures as Orry and Amelot, brings out the abilities and contributions of the little-known Comte

de Bergeyck, and pays proper attention to Melchor de Macanaz, the first of the great reforming ministers of the Bourbon period.

Some of Kamen's general conclusions may be summarized as follows: (1) The war had little effect upon population or population growth. (2) French influence on the ministers of Philip V has probably been overstated. Though French policy in the war was basically self-interested, it was relatively restrained, and the Spanish ministers of the new monarch were zealously national in aim. (3) The social structure of Spain was not significantly altered. The potentiality for change existed, but the Bourbons generally opted for mutual support of the status quo. Leadership did pass out of the grandee class into the hands of the lower nobility, however. (4) The newly acquired strength of the Crown was to be found in the consolidation of regalism, in the reform of the administration (especially the top echelon), and in "constitutional unification" accompanied by the simultaneous death of the remnants of representative government.

While possessed of many strengths, Kamen's book does have the defects of its virtues. Thematic unity is lacking. The book frequently seems to be a series of loosely connected parts rather than an organic whole. It is hard to grasp the interconnection of different factors, and for a narrative framework one must turn elsewhere. Nevertheless, balanced against careful, penetrating analysis and the presentation of new information that is solidly based on thorough archival research, these negative aspects have only minor impact. Kamen's book is not merely the first analytical work on the subject; it is fundamental to understanding the early Bourbon period in Spain.

Brigham Young University

GEORGE M. ADDY

HET KONINKRIJK DER NEDERLANDEN IN DE TWEEDE WERELDOORLOG. Volume I, VOORSPEL; Volume II, NEUTRAAL; Volume III, MEI '40. By *L. de Jong*. [Rijksinstituut voor Oorlogsdocumentatie.] (The Hague: Martinus Nijhoff. 1969. Pp. xi, 772; vii, 540; vii, 558. 41 gls.; 38 gls.; 45 gls.)

THE establishment of the Netherlands State Institute for War Documentation (*Rijksinstituut voor Oorlogsdocumentatie*) was one of the first actions of the Netherlands government on its return from London in May 1945. Louis de Jong, its first permanent director, had worked as a journalist in prewar Holland, and in May 1940 had escaped to London where he became a staff member of the Netherlands radio service. In 1955 he was asked by the Netherlands government to write a multi-volume history of the Netherlands during the Second World War. The present three volumes are the first to be published in a series, which is expected to include eight to nine volumes on completion.

The first volume, *Voorspel* (*Prelude*), after a brief treatment of some economic and political nineteenth-century themes, deals primarily with the First World War and the interwar years, particularly the Dutch right-wing and fascist movements, and the rise of National Socialism in Germany and its reverberations in the Netherlands. The second volume, *Neutraal* (*Neutrality*), begins with an extensive portrait of Queen Wilhelmina and proceeds to review the period of the "Phony War." It discusses in detail the changes in German campaign plans in the West, the "Venlo Incident," and changes in the Netherlands high command. The third volume, *Mei 1940*, (*May 1940*) deals primarily with the five-day war (May 10-14).

A study of these first three volumes of de Jong's work makes it clear that, despite its "official" character, this is the work of an individual historian combining personal

views and insights with a high level of sophistication and scholarship. By way of illustration, in a number of places de Jong takes issue with the conclusions of the report of the Parliamentary Commission of Inquiry. Also his assessments of the queen and the members of the government show a critical sense not handicapped by fear of censorship. This does not mean that the author may not have exercised some self-restraint with regard to living members of the royal house, or that he may not have de-emphasized certain matters of a more personal nature within the royal family, which were not of central importance to his story.

The author has been able to combine a great amount of detail with a wealth of personal judgment because of the variety of his sources and the great amount of time and team work that has gone into the book. He has used not only generally available source materials such as captured German documents and reports by other governments and commissions, but he also had access to special Dutch archives not generally open to researchers. In addition de Jong has made extensive use of personal interviews and personal letters, some presumably written in response to specific questions.

Using the materials on hand with great skill, de Jong has succeeded in clarifying some historical questions that previous research has failed to resolve fully. For example, the conflict between General Reynders, the Dutch commander in chief at the beginning of the war, and the government is explained judiciously. New light is shed on the circumstances under which queen and government made the crucial decision to leave the country on May 13, 1940, showing that this was not a concerted action as has often been assumed. The fact that General Winkelman's decision on May 14 to surrender to the Germans was based not so much on the bombing of Rotterdam as on the apparent determination of the Germans to bomb other big cities of the country, though not new information, receives a clear emphasis.

Two additional aspects of his work deserve special mention: De Jong writes beautifully; anecdotes, graphic descriptions of significant events, and broad judgments follow each other so that these scholarly volumes containing much minute detail become exciting and pleasurable reading; no doubt de Jong's experience with a variety of media stands him in good stead. And finally his portraits of some of the major figures of the period such as Queen Wilhelmina, Colijn, Mussert, and General Winkelman are masterpieces in which approval, even affection, and critical judgment are joined to give the reader a convincing and sometimes moving sense of the person whose portrait is being drawn.

The high quality of de Jong's latest endeavor leads me to express the hope that this series will become available to a wider reading public through translation. De Jong's work not only presents a segment of the history of the Second World War, which deserves to be read widely, particularly for the purposes of comparative history, but it can also serve as a model of historical method and style for historians everywhere.

Pitzer College

WERNER WARMBRUNN

NATIONAAL BIOGRAFISCH WOORDENBOEK. Volumes I-III. (Brussels: Koninklijke Valaamse Academiën van België. 1964; 1966; 1968. Pp. x, 984 columns; ix, 968 columns, xi; viii, 996 columns, xiv. 600 fr. B.; 600 fr. B.; 700 fr. B.)

IN 1938, with Volume XXVII (W-Z), the *National Biography* was completed in Belgium. After the First World War interest increased in expanding this series to include historical personages of the major Dutch language or of Flemish origin; and

the Royal Flemish Academy of Belgium took the lead. After more than fifteen years of work, three volumes have been published. The skill, learning, and insight of the many contributors results in a production obviously of more immediate appeal to specialists in sixteenth- to early twentieth-century cultural, political, and intellectual history of the Lowlands. The entire work is one illustration of the divided "two cultures or communities" in Belgium, but more importantly, of the intensified pride that the non-French Belgians have in their glorious past.

There is first of all a singularly large amount of material—numerous worthy and heretofore unknown reference sources and bibliographical suggestions—contained in the work of Professor Duverger and his associates. It is transparently clear that the general editors made a thorough and exhaustive search for competent scholars to write each entry, and that in their writings the individual authors displayed immense erudition. In general, the nine hundred plus essays are clear, detailed, and accurate. I read or sampled over a hundred of them, mostly those dedicated to individuals with whom the author was familiar. Those included, it must be noted, are relatively secondary figures since most Flemings of utmost importance were already covered in the earlier *Biography*. The most significant major individual covered in this three-volume addition is Alexander Farnèse, the governor of the southern provinces in the late sixteenth century. The subjects range from Alquerus, the eleventh-century architect, to Charles Catteau, the mid-twentieth-century ceramist. The selectivity process for inclusion is not stated, and therefore one might ask why not the poets Joost van den Vondel and Guido Gezelle, anatomist Jan Palfijn or composer Johannes Okeghem? More relevant criticism might deal with the length of the separate contributions. Is not the fourteenth-century mystic Jan van Ruusbroec overdone in fifty-four pages (I, columns 797–904)? What really cripples these tomes are the facts that brevity was not the goal of most of the contributors, and that the editors were too permissive. Cumbersome detail is very often not animated, and in a few cases—on Peene (II, 663–72) and on Tassis (II, 842–55)—it is trivial and petty detail, whatever the curious researcher may seek.

Volume III ends with a complete alphabetical index and Volume I commences with a listing of the contributors (all Dutch or Flemish except for Professor Raymond de Roover of Brooklyn College).

Tufts University

PIERRE-HENRI LAURENT

ENSEIGNEMENT PRIMAIRE ET ALPHABÉTISATION DANS L'AGGLOMÉRATION BRUXELLOISE DE 1830 À 1879. By *Herman Boon, P.B.* [Université de Louvain, Recueil de Travaux d'Histoire et de Philologie, Fourth Series, Number 42.] (Louvain: Bibliothèque de l'Université, Bureaux du Recueil; distrib. by Éditions Nauwelaerts, Louvain. 1969. Pp. xxxvii, 454. 600 fr. B.)

THE history of modern Belgium is conditioned by three great antagonisms: pro-versus anti-clericalism, central versus communal authority, and Flemish versus French culture. Anything of importance eventually got caught up in at least one of the three. Since primary education became entangled with them all, one would expect its history to be fraught with the clash of elemental forces and the passionate condemnations of embattled militants. But, alas, science and drama do not mix, and Professor Boon has produced here a tome with all the high excitement of a dried grasshopper collection in the Museum of Natural History.

His intent, nonetheless, is good. The book is a study of the development of Belgian

systems of primary education and an analysis of their effectiveness in combatting illiteracy. His research, furthermore, has been exceptionally thorough. He relies on a wide variety of primary sources and taps numerous special collections from over two dozen public and clerical archives. But above all he is painstakingly methodical—so much so that large segments of the book became little more than catalogues of material facts concerning each new school in each of his thirty-odd categories.

“Analphabeticism” is apparently a lower degree of illiteracy defined by the inability to distinguish between letters. It is a readily available statistical tool for historians, since the alphabetic could not sign his name to such documents as might be preserved in archives—Boon uses marriage license applications. Correlating them in a vague sort of way with data drawn from census reports and yearly militia call-up examinations, he establishes illiteracy rates for the various sections of Brussels and the six communes of its agglomeration at different periods. His problem, then, is to justify these statistical differences, using the local history of primary education. He makes no statistical correlations, however, and frequently explains things away in a most casual and sometimes unsatisfying manner. Absenteeism, geographical location of schools, poor physical facilities, even the use of French in teaching a predominantly Flemish population—he examines them all but finds that none is “a sufficient explanation for the differences in the evolution of alphabeticization in the Brussels agglomeration” (p. 418). He finally concludes that the only satisfactory explanation is a variation in the levels of material well-being (that is, poverty levels). This is probably true, but the conclusion loses most of its force from the fact that nowhere in the book has he examined the relationship between material well-being and education and illiteracy.

Lewis and Clark College

ALLAN H. KITTELL

GERMANS AND JEWS: THE RIGHT, THE LEFT, AND THE SEARCH FOR A “THIRD FORCE” IN PRE-NAZI GERMANY. By *George L. Mosse*. (New York: Howard Fertig, 1970. Pp. 260. \$8.95.)

THE title of this interesting volume is unfortunate. In the first place, it seemingly refers to a largely overworked but at the same time far too broadly conceived problem. Secondly, of the seven essays (including the introduction) the last three have only a tenuous connection with the Jewish issue. This lack of unity derives mainly from the fact that most of the essays were published previously in various yearbooks and anthologies. Fortunately, although of somewhat uneven quality, they have remained lucid and stimulating in their newly revised form. The introductory chapter deals largely with an analysis of the concept of “*völkisch*.” The fact that it is separated from racial-anthropological connotations adds depth to the discussion. The second and third essays pertain largely to the issue of anti-Semitism from the time of the Enlightenment to the end of the Second Empire, and in particular to the image of the Jew in popular literature. Neither of these well-structured sections breaks new ground, however. The fourth essay, “The Influence of the *Völkisch* Idea on German Jewry,” is far and away the best. In particular the analysis of the impact of the value system of the German youth movement on the Jewish youth movement is brilliantly and movingly demonstrated.

The following chapters, “The Corporate State and the Conservative Revolution in Weimar Germany” and “Fascism and the Intellectuals,” on the other hand, add little

to findings by such authors as Nolte or Klemperer. In the last essay, "Left-wing Intellectuals in the Weimar Republic," Professor Mosse intends to show a close relationship between the nonparty-bound Left in the Weimar republic and the New Left of today in this country. It is true that similarities between diverse intellectual trends do exist. Furthermore, it is the privilege and perhaps even the duty of the intellectual historian to focus attention on them. Nevertheless the analogies elaborated here look somewhat artificial and far-fetched. In itself, the value system of the Weimar left-wing intellectual is well perceived but Henrik De Man's theories expressed in *Die sozialistische Idee* (1933) and *Zur Psychologie des Sozialismus* (1926) with their stress on the ethical content of the movement rather than on the alleged vagueness of final goals are regrettably ignored. These books had great influence indeed on Left-oriented but not strictly party-bound German youth. The discussion of the impact of neo-Kantianism—fully legitimate in itself—cannot quite make up for this omission in a volume that has much to offer aside from and beyond what is indicated in the title.

Rutgers University

ROBERT A. KANN

LEBENSFORMEN UND DENKWEISEN DER AKADEMISCHEN WELT
HEIDELBERGS IM WILHELMINISCHEN ZEITALTER: VORNEHMLICH
IM SPIEGEL ZEITGENÖSSISCHER SELBSTZEUGNISSE. By *Helene
Tompert*. [Historische Studien, Number 411.] (Lübeck: Matthiesen Verlag. 1969.
Pp. 139. DM 24.)

TOMPERT's monograph, largely based on some twenty autobiographies of Heidelberg scholars and students, is brief and good. The first half contains two chapters on the students—their social, religious, and geographical origins, and the informal and formal aspects of student life—and two chapters on the professors—their formal *rites de passages*, duties, and privileges, and their informal socializing and intellectual circles. Particularly valuable are the descriptions of the "circles" around Max Weber, Stefan George, and the Wagnerian Henry Thode, and of the "colonies" of Polish and Russian students. Apparently the liveliest centers of intellectual activity among students as well as professors were these marginal men.

The penultimate chapter examines the relationship of the Heidelberg academic world to Wilhelmian politics, culture, religion, and morality. The final chapter compares Heidelberg with other German universities, and offers observations on the academy as a mirror of Wilhelmian society. Most trenchant is Tompert's view that the older university, based on the conservative, bureaucratic elite of *Geheimräte*, was slowly giving way to the liberal, but also romantic *Heidelberger Geist* represented by marginal figures like Weber and George. Given the contradictory forces they represent, Tompert's concluding warning—universities beware of substituting the preaching of a world-view for critical research—seems apt.

Simon Fraser University

ARTHUR MITZMAN

DEUTSCHLAND IM ERSTEN WELTKRIEG. Volume I, VORBEREITUNG, ENTFESSELUNG UND VERLAUF DES KRIEGES BIS ENDE 1914, edited by Fritz Klein; Volume II, JANUAR 1915 BIS OKTOBER 1917, edited by Willibald Gutsche; Volume III, NOVEMBER 1917 BIS NOVEMBER 1918, edited by Joachim Petzold. [Deutsche Akademie der Wissenschaften zu Berlin, Institut für Geschichte, Arbeitsgruppe Erster Weltkrieg.] (Berlin: Akademie-Verlag. 1968; 1968; 1969. Pp. xxvi, 514; xxi, 799; xx, 603. DM 25; DM 35; DM 25.)

IN spite of defects and excesses, this massive effort to set forth the history of Germany in the war of 1914–18 is a major achievement of historians in the German Democratic Republic. Those of us who read all 1888 pages may be few, but no scholar or serious student of the twentieth century can afford to neglect them. I do not know for any country in the Four Years' War a work to rival this one in scope, rich information, penetrating commentary, original contribution, and exploitation of so much, and so much recent, scholarship.

For a decade, articles and monographs—such as Kurt Stenkewitz's *Gegen Bajonett und Dividende* (Berlin, 1960) or Joachim Petzold's *Die Dolchstosslegende* (Berlin, 1963)—have appeared from the “work-group for the First World War” of the Institute for History in the German Academy of Sciences (Berlin), notably the interesting papers in *Politik im Krieg 1914–1918* (Berlin, 1964). The leader and general editor, Fritz Klein, also edited Volume I, on the origins of the war and its course to the end of 1914. Willibald Gutsche supervised the “collective of Marxist historians” for the longest volume, January 1915 to October 1917, and Petzold directed the final survey, November 1917 to November 1918. The editors' own sections amount to almost half of the elaborately documented account by some twenty writers. They cite archives in Potsdam, the Foreign Office in Bonn, Merseburg, Koblenz, Amsterdam, Budapest, Dresden, Erfurt, Ludwigsburg, Moscow, Munich, Stuttgart, Vienna, and microfilms from our National Archives. The historiographical essays that introduce each volume together provide a considerable monograph on writings on, respectively, the origins of the war, the politics of German war aims, and the ending of the war. Many points in the volumes as well as the regular belligerence towards “bourgeois historians” will be familiar to readers of the *Zeitschrift für Geschichtswissenschaft* or *Zeitschrift für Militärgeschichte*.

German imperialism is the theme of this history, devoted most extensively to domestic politics. German imperialism was more responsible than that of any other country for launching the war of conquest in order to redivide the world. The thesis that Germany from the beginning had no possibility of winning that war is expatiated on rather than examined. Writers could hardly attack the German ruling classes more devastatingly on these counts and also for the frightfulness in fighting the war and for selfishness and self-delusion in keeping the country in the struggle at the cost of such crushing losses for their own people and for others until the overwhelming defeat of Germany and her allies. Much evidence shows the extension and consolidation of control of the German economy by financial interests and big business, but the masses of often highly interesting information are often subordinated to political assertiveness and dogmatism, as well as illuminated by a class analysis of politics. Excoriations of Social Democrats and Independent Social Democrats sometimes surpasses that of the ruling classes. The Socialists betrayed the interests of the workers by supporting the war and working with the rulers so as to obscure its issues. The Independents, who receive the passion reserved for political rivals, are flayed for

relying on parliamentary struggle and seeking to develop parliamentarianism instead of pressing their opposition to the war and recognizing the need for revolution. Predictably, the heroes of the account are the small group of revolutionary socialists who opposed the war, often, to be sure, with much courage and self-sacrifice, and who came to seek the overthrow of the government. They do not escape criticism, however, especially for insufficiencies as measured by Russian examples. Surprisingly, "bourgeois pacifists" are treated not only at length but with praise for their opposition to the war and recognition of the wrongs of the imperial regime.

The discussions of war aims and diplomatic history are more than half as long as those of domestic politics, and the subjects are of course interwoven. There are some original contributions as well as scrutiny of scholarship, but the account largely follows Fritz Fischer's important contributions on the continuity of the efforts of the German ruling classes to conquer. Sections on warfare are brief and incisive, often highly cogent, as on the spring offensive in 1918 or the military defeat thereafter. Consideration of economic and social conditions is shorter than the sections on historiography and much focused on imperialist economic interests, but there are many statistics and other informative details, useful surveys, and cogent indications of the bitter squeeze and distress experienced by many Germans in the war.

The work suffers from the burden of its political purposes, since the writers are not only stronger but much more skillful in attacking historians or political figures or in elaborating political dogmas than in searching analysis or the reconstruction of persons' efforts and results. The two barren references to so important and fundamental a scholar as Arthur Rosenberg are shameful. The writers skimp Western scholarship, especially in English; the point is all the more serious because they are obviously familiar with much of it. While denouncing selfish and self-deluded German ruling classes and their socialist chauvinist lackeys, they seek to inquire neither why Germans in such numbers—like people in other countries—responded so simply to nationalist appeals, nor why Germans fought so long, so intensely, with such great sacrifices and destruction, nor why revolutionary efforts in Germany were so limited and so late. (They do not believe them to have been, and they do provide sympathetic and informative treatment of protest groups.) Although the authors point to the late arrival of Germany as an imperialist power and to the recent emergence of German finance-capitalism, their declarations provide neither a detailed nor a sufficient explanation of the widespread savagery of the Germans during the war and the domineering of the ruling classes. Dogmatic personification of imperialism substitutes for imaginative delineation of human beings with foibles and failures and possibly even strengths. Only some of the local leaders in the revolutionary socialist movement emerge as men of flesh and blood with recognizable pains and problems and zest in life.

In concentrating on domestic politics and ideologies in political conflict, the writers omit to inquire into the range and achievement of intellectual endeavor during the war, nor do they seek to depict the impact of the war on art and through art during the struggle or thereafter. Perhaps the most serious deficiency is that for all the mountains of information so little appears, or is sought, to show what the war was like for the Germans who fought it, who supported it, or who lived through it. Characteristically, the single reference to the moving diary and letters of Käthe Kollwitz merely asserts her hopes for justice in the world through the revolution in Russia in November 1917; nor do the useful photographs, forty per volume, provide any glimpse of the work of that fine, dramatic artist and sympathetic observer of Berlin workers, although the text refers to a political drawing in the last weeks of

the war. There is not even mention of works such as the letters of Franz Marc, the very publishing data of which bespeak the bitter pains of twentieth-century history. The great tetralogy of novels by Arnold Zweig, who died in Eastern Germany in 1968, will not easily be displaced for revelations of what the war was like for Germans. While these "Marxist collections" contribute useful history, historians of the Four Years' War have much yet to do, not least in Eastern Germany.

University of Washington

D. E. EMERSON

FRAUEN IM KRIEGSDIENST 1914-1945. By *Ursula von Gersdorff*. [Schriftenreihe des Militärgeschichtlichen Forschungsamtes, Beiträge zur Militär- und Kriegsgeschichte, Number 11.] (Stuttgart: Deutsche Verlags-Anstalt. 1969. Pp. 572, plates unnumbered. DM 38.)

It is appropriate that this extraordinary collection of documents dealing with the military role played by German women in two world wars should appear now that the movement for women's equality has reawakened. Indeed, the general neglect of this subject by scholars indicates how loath male historians are to admit the contributions made by women to what was once an exclusively male activity. Such neglect can no longer be excused after this publication in some five hundred closely printed pages of 270 documents culled painstakingly from the various war archives in Munich, Stuttgart, Ludwigsburg, and, of course, the federal archive in Koblenz. With this wealth of detail, no writer can be content to restrict a discussion of women in modern war to a few generalizations about war helping to "liberate" women.

Women were indispensable for the conduct of the wars of 1914-45. The documents in this collection show the development of the grudging recognition of this necessity in Germany. What emerges most clearly is the encumbrance provided by the traditional male ideology for the efficient waging of modern war. Even though, by 1939, German economists (unlike 1914) recognized the vital role of women in modern economies, before neither war did German political or military leadership plan in any significant way for the use of women in the military effort. Indeed, in both wars the incorporation of women was a sign of the increasing desperation of military effort. Indeed, in both wars, the incorporation of women was a sign of the increasing desperation of military leaders. But in contrast to America and England, extreme necessity never caused the German army to grant official military status to women, even those in uniform.

Several other topics are discussed in this important collection: the organized struggle of German women in both wars for male recognition of the services they could perform; the problems and confusions involved in the incorporation of women into the military effort; the proliferating duties of women from nursing to manning antiaircraft stations; and the particular struggle against Hitler's preadolescent views, which suffused Nazi ideology—the confusion of officials forced to reconcile the recruitment of women with Nazi ideas of male superiority, an ideology that made for administrative chaos and prevented the integration of the women's services into a single unit, makes for comic reading. We are given further documentary evidence of social discontent within the Third Reich—there were persistent complaints about the preferential treatment given upper-class women—and detailed statistical evidence about volunteers and draftees, their occupations and social origins. The wealth of information concerning the planning, organization, and establishment of the various women's services is invaluable for the historian of Nazi society and institutions.

Miss von Gersdorff has thus provided the documentation for a definitive study of "women at war" in Germany. Given her previous articles and good introduction to this volume, it is to be hoped that she herself will write such a work. My only criticism is the note of pride in the female military role that creeps into Miss von Gersdorff's prose. Women have proved themselves the equal of men in almost every area of modern life, and it is only to be expected that they should wish to do so in the art of warfare as well. That they should take pride in this achievement demonstrates that both sexes are indeed "human, all too human."

Rutgers University

HAROLD L. POOR

EVANGELISCHE KIRCHE UND PREUSSISCHER STAAT IN DEN ANFÄNGEN DER WEIMARER REPUBLIK: MÖGLICHKEITEN UND GRENZEN IHRER ZUSAMMENARBEIT. By *Claus Motschmann*. [*Historische Studien*, Number 413.] (Lübeck: Matthiesen Verlag, 1969. Pp. 147. DM 22.)

EVANGELISCHE KIRCHE UND DEMOKRATIE NACH 1945: EIN BEITRAG ZUM PROBLEM DER POLITISCHEN THEOLOGIE. By *Hans Gerhard Fischer*. [*Historische Studien*, Number 407.] (Lübeck: Matthiesen Verlag, 1970. Pp. 247. DM 36.)

THE affairs of the German Evangelical Church received much attention in the English-speaking world during the Nazi era. Pastor Niemöller, for example, came to be seen as a hero of Christian resistance against the Nazi totalitarian state, and the lively interest in the career of Bonhoeffer since the war has often reinforced this image. The very different traditions of German Protestantism and English liberalism were insufficiently appreciated, however. Two recent additions to the valuable series *Historische Studien* deserve consideration, for they throw more light on the tangled question of church-state relations and make it clear that the simple equations of former years need to be revised.

Claus Motschmann, a pupil of Professor Bussmann, has sought to challenge the view, expounded by Kupisch, Mehnert, and Dahm, that the clergy of the Evangelical Church in the Weimar period remained conservative, patriotic, and paternalist—interested in overthrowing both the injustices of Versailles and the democratic structures under which they were reluctantly obliged to live. He is concerned to stress the efforts at renewal that the church undertook, only to be repulsed by the ideological fixations of the Social Democrats, whose first actions in November 1918, under education minister Hoffmann, were nihilistically anticlerical. The hope of certain churchmen that disestablishment and the removal of the Kaiser would give the church its freedom were thwarted by the determination of the Socialists, and above all the DDP, to maintain state control of the church in the name of so-called "democratization." The attempt to reform the church from outside and above, not surprisingly, aroused strong resistance, not least on theological grounds. And it is significant that Westphalia, where orthodox theology was strongest, was to be the area of most opposition, both in the 1920's and again in the 1930's.

Motschmann shows a clear picture of the conflicts that arose between those who wished to initiate a new and democratic church in tune with the new needs of society, and those who sought to defend the integrity of its theological witness above and beyond passing political or social conditions. The crisis, which in England occurred over prayer book revision, here took on wider and deeper implications. Motsch-

mann's able analysis of the records of the Prussian church gives valuable insights into the background of the church-state conflicts and of the rival understandings of what the church should be and do, which were to divide the German Evangelical Church irreparably in the following decade.

In the circumstances of physical destruction and moral shame following the end of the war in 1945, the issues of church and state became even more acute. H. G. Fischer's excellently scholarly contribution outlines clearly the debates that ensued between those who sought to use the opportunity of political collapse to reform and refound the church on a basis of repentance for past errors and an outright commitment to democracy, and those who sought to create a conservative bulwark against the dangers of revolution and communism. His sympathies are clearly with the former group, and he is strongly critical of the missed opportunities that paved the way for the restoration, in both church and state, of conservative and hierarchial patterns, so well exemplified in the paternalist regime of Dr. Adenauer. He has added not merely a historical narrative but an extensive analysis of the theological positions adopted amongst West German theologians in defense of these traditional positions. His criticism of the Lutheran readiness to come to terms with the "establishment" in the present West German republic provides interesting parallels and contrasts with the reluctant process of accommodation described by Motschmann in the circumstances of the preceding generation.

University of British Columbia

JOHN S. CONWAY

THEODOR HEUSS UND DIE WEIMARER REPUBLIK: EIN BEITRAG ZUR GESCHICHTE DES DEUTSCHEN LIBERALISMUS. By *Modris Eksteins*. [Stuttgarter Beiträge zur Geschichte und Politik, Number 3.] (Stuttgart: Ernst Klett Verlag. 1969. Pp. 204. DM 32.)

DER CHRISTLICH-SOZIALE VOLKSDIENST: VERSUCH EINER PROTESTANTISCHEN PARTEI IN DER WEIMARER REPUBLIK. By *Günter Opitz*. [Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, Number 37.] (Düsseldorf: Droste Verlag. 1969. Pp. 371. DM 48.)

THESE books address themselves to one of the causes often cited for the Weimar Republic's demise—the fragmentation of the German electorate into a multitude of splinter parties. Both studies were originally written as dissertations and are therefore quite narrow in scope. But since they contain much fresh material, they are also useful. The Canadian historian Modris Eksteins has written the shorter but more interesting book. It centers on Theodor Heuss, prominent member of the German Democratic party (DDP) during the Weimar years and president of the German Federal Republic after World War II. Eksteins presents a good brief survey of the DDP's decline from mainstay of the moderate Left in 1919 to mere splinter party (renamed *Staatspartei*) in 1933. His picture of Heuss, while favorable, is not uncritical. On the plus side, Eksteins stresses Heuss's genuine concern with breathing life into the concept of "national democracy." As journalist, Reichstag deputy, and teacher at Berlin's *Hochschule für Politik*, he tried valiantly to counteract the claim of the antirepublican Right that democracy was a Western import alien to Germany. On the minus side, Eksteins points to Heuss's Francophobia, his hatred of Stresemann, and his ambivalent attitude toward the Social Democratic Party (SPD) as major obstacles to a policy of compromise that might have helped consolidate the forces of moderation.

The book by Opitz on the *Christlich-soziale Volksdienst* is more detailed. The CSVD was a merger in 1930 of two small Protestant factions—the South-German *Christlicher Volksdienst* and the *Christlich-soziale Reichsvereinigung*—that had split off from the German National People's Party (DNVP). The CSVD started off promisingly with fourteen deputies; but by 1932 these had dwindled to three. Its major interest derives from the fact that it was the only strictly Protestant party in the Weimar Republic. It was not, however, the only or even the major party attracting Protestant votes; as in the past, the DNVP continued to do that. The added drawback, that the CSVD had neither the full support of the Protestant churches nor of the clergy, helps account for the party's weakness, as did the lack of outstanding leaders, a powerful press, and funds.

Both DDP/*Staatspartei* and CSVD supported the Weimar Republic and thus were caught in the cross fire of radical extremists from Right and Left. Both, in different ways, hoped to bridge the gap between the middle and lower classes by concentrating on domestic reforms. They both stood in the shadow of Friedrich Naumann, Heuss's mentor and friend; yet they never asked themselves whether Naumann's old-fashioned blend of nationalism and socialism was really the best antidote to Hitler's heady brew of national-socialism. Neither the *Staatspartei* nor the CSVD really understood what Nazism was all about, though in this they were hardly unique. Schemes to join forces with other moderate groups were considered, but only halfheartedly. To collaborate with the SPD was unthinkable to the leading conservatives in both parties. The *Staatspartei* preferred to remain "small, but pure." Small it certainly was, but pure? When Hitler asked the Reichstag for absolute powers, the *Staatspartei* deputies gave in, including Heuss. He would have preferred to abstain but he bowed to party discipline. The CSVD went further and expressly endorsed the Enabling Act of March 23, 1933. Only the SPD voted against it.

Yale University

HANS W. GATZKE

WIDERSTAND—STAATSSREICH—ATTENTAT: DER KAMPF DER OPPOSITION GEGEN HITLER. By *Peter Hoffmann*. (Munich: R. Piper & Co. 1969. Pp. 988. DM 58.)

THE GERMAN RESISTANCE TO HITLER: RESISTANCE THINKING ON FOREIGN POLICY, by *Hermann Graml*; SOCIAL VIEWS AND CONSTITUTIONAL PLANS OF THE RESISTANCE, by *Hans Mommsen*; RESISTANCE IN THE LABOUR MOVEMENT, by *Hans-Joachim Reichhardt*; POLITICAL AND MORAL MOTIVES BEHIND THE RESISTANCE, by *Ernst Wolf*. Introduction by *F. L. Carsten*. (Berkeley and Los Angeles: University of California Press. 1970. Pp. xix, 281. \$7.95.)

MR. HOFFMANN, who currently teaches history at the University of Northern Iowa, reaches no startling new conclusions in his massive monograph, but his painstaking research permits authoritative judgments on the details of his predecessors' work and adds significantly to our factual knowledge of the German opposition.

In tracing prewar efforts to remove Hitler, Hoffmann insists on the crucial importance of the Munich Agreement, arguing that by September 1938 the conspiracy was so widespread and strong that it might well have changed the course of history if only outside forces had cooperated. Instead, the West rescued Hitler at Munich and gave those who sought to overthrow him "a blow from which they could not recover."

The author describes some seven carefully planned attempts to kill Hitler and follows the frustrations and failures of each in excruciating detail. Using the brilliant historical detective work of Anton Hoch, he agrees that the bombing of the Bürgerbräukeller on November 8, 1939, was not planned by the SS for purposes of propaganda; it was done by a loner, the intrepid cabinetmaker Georg Elser.

The main focus—indeed, almost half the book—is placed on the Stauffenberg conspiracy. While giving due credit to such studies as those of Kramarz, Ritter, Schramm, and Zeller, the author uses his own impressive and imaginative research to correct previous misunderstandings and to provide new information with regard to plans, dates, timing, and personnel. He differs sharply from the conclusions of Wheeler-Bennett and others that General Fellgiebel, a conspirator in charge of military communication, was chiefly to blame for the collapse of the July conspiracy by failing to blow up the communication center at the *Wolfschanze*. Hoffmann defends Fellgiebel convincingly, arguing that such demolition would have been “first, impossible and second, senseless.” A footnote of 71 lines establishes that Colonel Brandt did not in fact push Stauffenberg’s briefcase to another side of the table support. Another note of 78 lines shows why it took Stauffenberg two hours to fly from Rastenburg to Berlin. We learn that Hitler wore long white underwear on that day, even though the heat was oppressive, and another 96 lines of footnote give sources for the exact wording and timing of the first dispatches from the Bendlerstrasse sent out by the conspirators. The failure of the putsch is traced meticulously—often hour by hour—in Berlin, in all twenty of the *Wehrkreise*, in Prague, in Vienna, and in Paris. The torture and death of the conspirators are described with horrifying precision.

The book is not without faults: there is no overall concluding appraisal; there is redundancy of phrase and repetition of detail; in spite of the comprehensive title, such resistance groups as the White Rose and the *Rote Kapelle* are given short shrift, and the treatment of Fromm and Kluge seems too charitable for such despicable opportunists. Nevertheless, this dense volume with its two hundred pages of closely argued footnotes and extensive use of archival, manuscript, and oral testimony, fulfills the author’s ambitious goal of writing the most completely documented narrative history of the German resistance movement to date. Whatever else is read on the subject, Hoffmann’s indispensable reference work must also be consulted.

The essays, each written by a German specialist, discuss important problems of the German resistance with judgment and candor and offer the kind of interpretive judgment often lacking in Hoffmann’s narrative history.

In his strikingly frank article on foreign policy, Graml shows that Goerdeler and the conservatives were never quite able to reconcile their hopes for a supranational solution in central Europe with their desire to fulfill traditional national aims from a position of German strength. The author concludes that their *Ostpolitik* was anachronistic and unlikely to have achieved the stability they sincerely wanted: “The revival of the imperial concept would have been intolerable to Europe . . . There was never the remotest chance of making peace with Western powers on such a basis” (p. 25). Conservatives of the resistance agreed with Hitler about the necessity for territorial revision of Versailles. They broke with him over the means he used, for they were appalled both by Hitler’s determination to resort to war and by his pact with Bolshevik Russia.

In discussing social views of the resistance leaders, Hans Mommsen concludes that their central purpose was the “de-massing of the masses” (*Entmassung der Masse*). They rejected both communism and Western democracy. As humanist elitists who were much influenced by Nietzsche and Jesus, they emphasized the importance of

human personality rather than universality or individuality, and they sought an order that would provide the conditions for personal, intellectual, and religious development. In economic policy the Resistance was split. Goerdeler attacked the Kreisau circle as "drawing-room Bolsheviks"; they, in turn, derided Goerdeler as a neo-Brüning who roundly damned Keynes, the New Deal, and "the Marxist theory of the 8-hour day." All resistance groups found it easier to set forth ideal goals than to plan the institutions for their realization.

As Reichhardt notes in his essay on the difficult topic of resistance in the labor movement, the lack of source materials makes it unlikely that a thorough history of German labor's opposition to Hitler can ever be written. In his thoughtful essay, however, the author has helped greatly to increase our knowledge of the problem. Communist leaders come out badly here. They failed utterly to understand the threat of Hitler, and in their blind hatred of the SPD refused to join in efforts to thwart his coming to power. And after 1933 Communist policy as dictated by the Kremlin was totally ignorant of German conditions. It continued to demand that the party members distribute the hackneyed Communist propaganda produced in Moscow. Loyal Communists who followed such orders were easily rounded up by the Gestapo. In numbers larger than any other resistance group, thousands of Communist workers suffered unspeakable torture and death. Other opposition organizations such as the *Neu Beginnen* are also treated briefly but effectively.

In a survey of the well-worked field of resistance to Hitler on moral and religious grounds, Ernst Wolf concludes, as have so many others, that the heroic resistance of individual Christians contrasts lamentably with the role played by organized Christianity. Both the Protestant and Catholic churches hailed Hitler as a savior from atheistic communism; both preached obedience to secular authority; and neither took a firm stand against the regime, which was indeed, in Gladstone's phrase, "the negation of God erected into a system of government."

These thoughtful essays reinforce the impression gained in larger and more detailed studies: the resistance to Hitler's barbarism by decent German citizens was widespread and genuine—and tragically ineffective.

Williams College

R. G. L. WAITE

ECONOMIA E SOCIETÀ IN PIEMONTE DALL'UNITÀ AL 1914. By *Valerio Castronovo*. [Studi e ricerche di storia economica italiana nell'età del Risorgimento.] (Milan: Banca Commerciale Italiana. 1969. Pp. 409.)

THE present volume in the series of studies on the Italian economy sponsored by the Banca Commerciale Italiana concentrates upon developments in Piedmont from unification to World War I; it is divided into three parts. Part I deals with the influence of Cavour's free trade policies and the initial impact of the French market upon the area, Part II examines the cost and consequences of Piedmont's incorporation into the national market, and Part III describes the region's industrial spurt forward.

In the first part Castronovo shows that twenty years after unification, Piedmont preserved an agrarian economy characterized by the persistence of peasant proprietors. Indeed he contends that the compulsive drive of the peasantry to become landowners funneled savings into mortgages that rendered difficult the accumulation of capital for improvement and innovation. Hence his findings support the case made by Romeo in his *Risorgimento e capitalismo* (1959) that fragmentation of the land—so ardently desired by democratic reformers—worked to arrest Italy's evolution of a more modern

economy. Castronovo demonstrates that free trade conspired with the system of small holdings to retard Piedmont's economic growth.

In the second section the author catalogs the series of events—agrarian crisis, tariff war with France, speculation in the building industry, and the failure of the credit institutes—that broke the back of the Piedmont's traditional economic structure. Relying upon the available statistics, he describes the disruption in the countryside and the dislocation in the silk industry at the turn of the century. Financially it provoked a re-allocation of capital, which the author believes paved the way for the growth of the cotton industry. He views protectionism positively, indicating that the tariffs of 1878 and 1887 assured the cotton industry a slice of the home market and served to attract Swiss, German, and domestic capital. This capital, he insists, provided cotton producers with a modern corporate base and the means to mechanize. In turn, the expansion of the cotton industry stimulated the economy of the entire area, providing the preliminary conditions for the industrial "take-off" in Piedmont.

The concluding chapters dwell upon the factors that facilitated Piedmont's economic transformation in 1905–10, paying particular attention to the policy of industrial promotion adopted by the municipality of Turin and the influx of corporate capital into the automobile industry. The interpenetration of industry, agriculture, and finance capital is examined, as well as the modifications this brought about in the countryside. These same pages provide an analysis of the changes produced in labor-management relations and in the region's political life.

Interestingly enough, Castronovo ties the success of Giolitti's industrial democracy and social reformism in the Piedmont to its economic development. This, like most of his other observations, is well documented by a series of sources drawn from private and public archives. In addition to the invaluable information about the process of modernization in Piedmont, the work contains important insights applicable to the peninsula as a whole. Consequently the specialist who strives to overcome the author's cumbersome style of writing will be well rewarded by reaching the valuable material contained in this study.

Brooklyn, New York

FRANK J. COPPA

Near East

L'ISLAM ET LA CROISADE: IDÉOLOGIE ET PROPAGANDE DANS LES RÉACTIONS MUSULMANES AUX CROISADES. By *Emmanuel Sivan*. (Paris: Librairie d'Amérique et d'Orient Adrien Maisonneuve. 1968. Pp. 222. 46.50 fr.)

THIS is a splendid book on an apparently unpromising subject, and is highly recommended to all serious students of the Crusades. By systematically following the Koranic prescription of *jihād*—eternal war against the enemies of Islam, to defend and extend its frontiers—as preached and practiced in Syria and Egypt from the seventh-century conquest to the expulsion of the Franks in 1291, M. Sivan has thrown clear and convincing light on hitherto unexplained fluctuations and reversals in rulers' policies and their popular support, culminating in the Mamluk sultans' successful "multiple *jihād*" against the menacing Mongols, their Armenian allies, the Franks on the coast, and even the Nubians.

His exhaustive scholarship is evidenced not only by thorough documentation and judicious annotation and by an excellent bibliography, but particularly by his skilled use of a wide range of materials, both published and unpublished. He traces the decline in belligerence in Syria before 1097, the panicky but nonideological reaction to the

First Crusade, the establishment of a *modus vivendi*, which crumbled after 1118 because of Frankish greed and bigotry, and the initial reconquest (of Edessa in 1144) by Zengi, whose motives and achievements were exaggerated by propagandists.

With Nur-ad-Dīn (1146–74) we start the “decisive phase” of intensive propaganda, stressing the sanctity of Jerusalem and Moslem unity against Frankish interlopers; Saladin (1174–93) built skillfully on this basis to achieve virtual success, only to lose portions to the Third Crusade. His Aiyūbid successors paid lip-service to the *jihād* ideal but were so venal and apathetic that the vigorous and pious Mamluks, aided by religious leaders and popular enthusiasm, easily overthrew them and engineered their subsequent triumphs.

Space is lacking to detail the ramifications of the author’s modest but convincing thoughts—on native Christians, on juridical and epic literature, on Sunnite-Shi’ite and Shafi’ite-Hanbalite disputes, on individual personalities and dynastic “images”—but all are carefully analyzed, judiciously qualified, and thoroughly documented—in a word, impeccable.

Princeton, N. J.

HARRY W. HAZARD

HISTOIRE DES PRIX ET DES SALAIRES DANS L’ORIENT MÉDIÉVAL. By *Eliyahu Ashtor*. [École Pratique des Hautes Études—VI^e Section. Centre de Recherches Historiques. Monnaie, prix, conjoncture, Number 8.] (Paris: S.E.V.P.E.N. 1969. Pp. 572. 98 fr.)

BEFORE economic historians can systematically analyze medieval European or Islamic economic history, the scattered data from a myriad of sources must be gathered, compared, and put into some rational, comparable form. Professor Eliyahu Ashtor’s massive study of prices and salaries in Iraq, Egypt, and Syria with comparative data for the northern coast of the Mediterranean represents a major step toward that goal. Not only has this Israeli scholar combed the Arab chronicles, geographies, and encyclopedias for their relatively infrequent and isolated references but he has also included very important data from the Geniza documents, from European accounts, from travelers, and, for the first time on a large scale, from the archives of Venice, Ragusa, and Prato. Each chapter—two on Iraq to the Mongol conquest, two on Syria to the Ottoman conquest, three on Egypt to the Ottoman conquest, and the conclusion, which gives the data on the European coast of the Mediterranean—follows a similar pattern. First there is a general statement on social and political conditions, then a brief summary of the monetary system and a listing of all of the available price data for wheat, barley, bread, spices, textiles, rents, and salaries; also there is a survey of the cost of living and, finally, an account of salaries. While Professor Ashtor’s data are not absolutely complete, it is unlikely that any new data, particularly for the fourteenth and fifteenth centuries where the Arabic and European archival material is so rich, will indicate fundamentally different economic patterns.

However, it is just this concern for thoroughness that has led to a number of reservations about the use of this work. A number of Professor Ashtor’s important ideas as to why certain price trends prevailed—demographic changes and specific military factors, for examples, are buried between sentences filled with numbers and percentages. While there are numerous tables that list the data by date, price, and source, many of these tables lack complete titles for easy reference, and there is no general list of them. With the exception of the chapter on Mamluk Egypt, there is no easy way to translate the prices in the tables and text into comparative units of gold, as it is not always clear

from the section on the monetary system which is the appropriate exchange rate for a given date. While the extensive footnotes reflect the wide range of medieval and modern sources used by Professor Ashtor, the "bibliography" leaves out many of these modern works and is, in fact, a list of abbreviations arranged by type of source and not alphabetically.

All of this is meant as a warning to the proverbial general reader and even the student of Near Eastern history that this is not an introductory survey. In fact, Professor Ashtor's work must be considered an essential source for the scholar of medieval Near Eastern and European economic history who wishes to investigate in depth the changes in prices and salaries in the medieval Islamic and Mediterranean world.

University of Washington

JERE L. BACHARACH

THE EMERGENCE OF THE MIDDLE EAST: 1914-1924. By *Howard M. Sachar*. (New York: Alfred A. Knopf. 1969. Pp. xiii, 518, xxix. \$12.50.)

SEASONED travelers have a rule of thumb that hotels named "The Central Palace" are usually neither central nor palatial. The word "emergence," much in vogue in titles of historical works, is getting to be equally misleading. Professor Sachar's popularized account of the decade 1914-24 is more concerned with the demise of the old Ottoman Empire than it is with the "emergence" of the new Middle East. His limitation of the Middle East to "the lands and peoples of Western Asia" seems pedantic. It does, however, allow him to finesse the problems posed by Egypt and to say little about Lebanon and Syria, while devoting most of his attention to Turkey, Iraq, and Palestine. He also includes a great deal of pedestrian powder-and-shot military history, complete with mislabeled maps.

Professor Sachar's style is flowing, but his torrent of words provides few new insights. The trusty old clichés, somewhat dressed up with new adjectives, about the inefficiency of the Ottoman Empire and its decline still shine through. Here, as in other cases too, we are dealing with a historian whose cultural values are largely Eurocentric, and who has no intimate knowledge of Ottoman history or of the Turkish and Arabic languages. His control of Turkish geography is also defective. The Golden Horn and the Bosphorus are not interchangeable terms (Chapter 1), and even in Mustafa Kemal's early days Ankara was not separated from Constantinople by "two hundred miles of harsh mountainous terrain" (p. 415).

Although Professor Sachar casts words about with abandon, he fails to come to grips in psychological terms with any of the actors in his large cast of characters. He speaks of "Berlin's coup in maneuvering the Ottoman Empire into the war" as "a stupendous one" (p. 33). Such a view is only possible by neglecting the personality of Enver Pasha. Similarly, Mustafa Kemal's selection of Ismet Pasha to head the Turkish delegation to Lausanne is far more complex than Sachar's view that Ismet possessed the qualities of toughness and loyalty (pp. 448-49). Anyone can make mistakes, but this book conveys the distinct impression that style is more important than accuracy, and that how one says something matters more than what one says.

Surveys have their place, but in the Middle Eastern field the need is for basic research rather than for syntheses of other syntheses. One is hard pressed, therefore, to find justification for the publication of this work. Professor Sachar does emphasize the long-standing Russian interest in this part of the world, but that is neither new nor sufficient.

Princeton University

NORMAN ITZKOWITZ

JEWES IN NINETEENTH-CENTURY EGYPT. By *Jacob M. Landau*. [New York University Studies in Near Eastern Civilization, Number 2.] (New York: New York University Press. 1969. Pp. xvi, 354. \$12.50.)

WITHIN the period covered by this volume the Jewish population in Egypt increased more than eightfold—from an estimated seven thousand during the first half of the nineteenth century to nearly sixty thousand in 1917. Immigration, the main factor in this growth, was due to flight from persecution and general insecurity in Russia, Rumania, Morocco, the Yemen, and Turkish lands as well as to the pull of economic opportunity and equal legal status that British control brought in its train. By the time of World War I, a number of Jewish communities of importance had developed, notably in Cairo and Alexandria. The evolution of these communities is the theme of the book.

Archival material in Egypt has been unavailable to the author, a member of the faculty of the Hebrew University of Jerusalem. He has, however, made good use of printed sources and information gleaned from archives in Jerusalem and the Alliance Israélite Universelle in Paris and from the British, French, and Italian foreign ministries. From these fragmentary and fugitive sources, he has sketched the outlines of the demographic and occupational patterns, the religious and cultural life, and the philanthropic efforts of the Jewish communities. The book contains a text of 125 pages and a valuable collection of documents in Hebrew, Arabic, and the European languages. The picture that emerges is that of a heterogeneous and fragmented population struggling against heavy odds to strike root in an unfriendly environment and to maintain its religious and cultural identity. Whether and to what extent Egyptian Jews were involved in British and French economic imperialism has not been probed, doubtless because of the unavailability of the necessary source material.

The traditional status of Jews in a Moslem state was one of inferiority and insecurity. British control brought legal equality, but the time-honored social distance and hatred persisted. As a result, the Jews remained an apolitical element, and many of them, like other foreigners, retained their old citizenship or sought the protection of European powers.

Toward the end of the nineteenth century, a change was evident in the traditional occupational structure of the large majority. In addition to peddling, small financial operations, and crafts, Jews found employment as clerks in foreign commercial enterprises and the consulates, and some entered the liberal professions. The Oriental pattern of a few wealthy families and a precarious livelihood for the many remained, however.

Throughout the period, religion was the distinctive mark of Egyptian Jews, but Western influence caused erosion in religious observance. The communities maintained their synagogues and charitable institutions. The education of the children was a continuous struggle, with frequent appeals for aid to the Alliance Israélite Universelle. The curriculum, which emphasized languages evidently as a means of livelihood, encouraged a shallow and Levantine education. Culturally, Egyptian Jewry of the nineteenth century was quite barren. Western influence loosened the foundations of tradition but did not serve as a European cultural leaven among the masses of Egyptian Jews.

City College of New York

OSCAR I. JANOWSKY

A MODERN HISTORY OF SYRIA: INCLUDING LEBANON AND PALESTINE. By *A. L. Tibawi*. ([New York:] St. Martin's Press. 1969. Pp. 441. \$10.95.)

THE three sections into which this work divides itself are quite different in intrinsic qualities. The last, and by far the shortest, treats Syria since 1920. Tibawi's view of the French mandatory regime in Syria (Lebanon and Palestine are excluded) is that of an Arab nationalist. When writing of recent events, however, his Nasserist sympathies lead him to opinions of the Syrians very similar to those expressed by the French colonialists.

The opening section, which comprises one-half the book, is an excellent general history of geographical Syria from the end of the eighteenth century to 1914. Of special importance is the analysis of how the policies of the European powers, the activities of missionaries, and the Turkish reforms led to the development of religious antagonisms and the emergence of Arab hostility to both the West and the Ottoman state. The author's emphasis on Ottoman state and Moslem private educational activity and his description of 'Abd al-Hamid II's reign are notable advances over the usual treatments. Not surprisingly, in a work of this breadth, some of the author's judgments are questionable. One such is his view that Sharif Husein was exiled to Istanbul as "a form of check on Arab claims to the Caliphate" (p. 171), a judgment that foreshadows the approach in the remaining division of the book.

In the middle section, Tibawi offers a new study of Anglo-French policies toward geographical Syria during 1914-20, which, unlike hitherto published accounts, is based on full use of the unpublished British official records. Writing in the tradition exemplified by Antonius, the author depicts Husein as motivated solely by Arab nationalism from beginning to end and as the central figure who possessed a mandate from the Arab nation. The author does not explicitly claim unmitigated British duplicity, as other writers in the tradition have, but he uses his evidence, often obscurely, to identify British knaves or fools. What his evidence does show is that there was great divergence of opinion among British officials, as existing studies have already shown. I see no reason to change any of my views concerning Husein and the Arab revolt or to modify the general conclusions of Kedourie and Zeine regarding Anglo-French relations with the Hashimites. These are admittedly difficult questions. Moreover, the doubts expressed here may arise from the Tibawi's compression of the material in order to present it as a portion of a general history. It is to be hoped that he will present the results of his researches more fully in his promised work on "Anglo-Arab relations concerning both parts of Palestine."

University of Illinois, Urbana-Champaign

C. ERNEST DAWN

THE CAMBRIDGE HISTORY OF IRAN. Volume V, THE SALJUQ AND MONGOL PERIODS. Edited by *J. A. Boyle*. (New York: Cambridge University Press. 1968. Pp. xiii, 762. \$12.50.)

THIS is the first of the projected eight volumes of this series to appear. "The aim," according to the jacket, "is to provide a collection of readable essays rather than a catalogue of information. . . . It is hoped that the volumes will act as a stimulus to specialists; but they are primarily concerned to answer the sort of questions about the past and present of Iran that are asked by the non-specialist."

By this canon much of the volume is disappointing, valuable and welcome though it is. How can it be otherwise when over forty-seven per cent of the text is given to political and dynastic history, unless the two authors are paragons of writing skill? For the Mongols the editor comes nearer to an acceptable balance, but the Saljuq

chapter is far too long (202 pages) and packed with detailed reference information. Most of this half of the book should have appeared in monographs, with these chapters a fraction of their length, viewing the significance of the forest instead of losing the lay reader among the trees. In the forest are hidden fine brief interpretations; but they are scattered, unconnected, and never set in any bas-relief pattern to attract the attention of the nonspecialist. One asks if the editor might not have wrought better balance.

The handling of the sources is competent and judicious. Yet many readers will regret that there is not some detailed critique of the sources and attendant problems, either in the introduction or the appendix.

The next longest essay (Chapter 2), "The Internal Structure of the Saljuq Empire," is the competent, integrated, and structured discussion we would expect from Professor Lambton. Here and there the non-specialist may get bogged down amidst detail, but the sense of authoritative, structured treatment should sustain him to the next, more selective discussion.

The rest of the volume will be more to the taste of the educated lay reader, yet also highly appreciated by the specialist.

For the Mongol period social history is concerned with general socioeconomic conditions in comparative contrast to Saljuq—a good summary, with adaptations, of his relevant book in Russian by Professor Petrushevsky of Leningrad. One wishes he would not use the term "feudal" so loosely, as though in all respects it paralleled medieval Europe; and that the litterateurs, especially poets, be more laid under tribute for the enrichment of social history. Persian historians generally have yet to exploit the wealth embedded in literature. The new generation must learn to correct this.

The interested reader will enjoy the superb chapter by Professor Rypka on the poets and prose writers of the combined periods, from Sana'i to Sa'di, and beyond, including the great mystics. Here is a fascinating fusion of social and intellectual history, to point the way for future efforts.

Professor Bausani's essays on religion are balanced and perceptive, but regrettably short. We may be grateful that the late Professor Hodgson of Chicago, before his untimely and lamented death, provided his mature interpretive summary of Iranian Isma'ilism in this period. Stimulating and interesting are the concluding essays by Professors Grabar and Kennedy on the visual arts and exact sciences, which are in this time creatively important, not only in Iran but the world at large.

The volume is an overdue and distinctive contribution to Iranian studies. It is cordially to be commended, with the understanding that the first half is more profitable for special reference and the second for both illuminative and pleasurable reading.

University College, Nairobi

T. CUYLER YOUNG

THE ARMENIANS. By *Sirarpie Der Nersessian*. [Ancient Peoples and Places, Volume LXVIII.] (New York: Praeger Publishers. 1970. Pp. 216. \$8.50.)

For placing a volume on the Armenians in this long and scholarly series, its general editor, Dr. Glyn Daniel of Cambridge University, deserves credit. And he was fortunate in having it written by the most eminent scholar in the West on Armenian studies. The result is a learned and lively account touching on all aspects of Armenian life from the earliest times to the end of the fourteenth century. In addition to the part on history, there are chapters on the social and economic life, religion, literature and learning, architecture, sculpture, and painting (including manuscript illumination).

Before the dawn of history the Armenian Highlands were inhabited by cavemen

and hunters. The earliest written records (Hittite tablets) date from the fourteenth century B.C. In the beginning of the Urartu kingdom in the ninth century B.C., and with its quick rise as a mighty state, some scholars see the ancestors of the Armenian people, though they are not known as such until the late sixth century B.C. (in the Behistun inscription of Darius). The origin of the name of the country remains obscure, though many assert (mostly on philological grounds)—and our author concurs—that the Armenians are an Indo-European people of unknown provenance (p. 20). As Armenians they first lived under Persian rule, then under Seleucid kings, and finally early in the second century B.C. they founded the Armenian kingdom. With cataclysmic political ups and downs, often as client of Persia and of Rome, the kingdom continued until 428 A.D., when most of the country again fell under Persian suzerainty. Even then the Armenians retained their distinct national identity, mostly by their religion and language. These twin forces were instrumental for the survival and medieval blossoming of the Armenian culture, especially its arts and literature, whose importance is now being appreciated by the world of scholarship.

Every page of the book testifies to thorough research in printed and manuscript Armenian sources, fortified by full familiarity with the best recent studies in major Western languages and in Russian. Besides some fifty drawings in the text, the book is enriched with black-on-white plates of illuminated manuscript pages and of monumental architecture. There is an excellent bibliography and a good index.

Library of Congress

A. O. SARKISSIAN

COLOPHONS OF ARMENIAN MANUSCRIPTS, 1301-1480: A SOURCE FOR MIDDLE EASTERN HISTORY. Selected, translated, and annotated by *Avedis K. Sanjian*. [Harvard Armenian Texts and Studies, Number 2.] (Cambridge, Mass.: Harvard University Press. 1969. Pp. xv, 459. \$10.00.)

In recent years, the silence covering the history of Armenia and eastern Asia Minor in the chaotic centuries leading up to the establishment of Ottoman hegemony has been partly broken by the publication in Soviet Armenia of the *Minor Chronicles of the XIII-XVIII Centuries* (1951, 1956), the three volumes issued to date of the *Corpus of Armenian Inscriptions* (1960-), and the four volumes of the *Colophons of Armenian manuscripts* (1950-67) covering the period 1301-1500, brought out by Ē. Xačikyan, the director of the Armenian State Library (Matenadaran). In view of the dearth of other historical sources, the varied evidence of the colophons, with their vast material on chronology, topography, demography, personal names, and administrative and economic terminology, becomes of particular importance, not only for Armenian specialists, but for all scholars concerned with the numerous peoples and principalities found in the area: Seljuks, Mongols, Turkomans, and Kurds, for examples.

Professor Sanjian's book is a translation of the historical passages found in the colophons of the first three volumes of Xačikyan's work (all then published), followed, somewhat unexpectedly, by a translation of two documents of a different genre—the "Laments on the Fall of Constantinople" by Aṙakel Bałiřec'i and Abraham Ankiwraç'i. These translations are preceded by a useful introduction on Armenian colophons and the multiple calendrical systems used by the scribes. Rather than overburden his text with footnotes, which are relatively few in number, Professor Sanjian has followed Xačikyan's lead and provided extensive appendixes identifying persons, peoples, places, and scribes as well as Scriptural references and foreign terms. A multilingual bibliography and a map complete the work.

Throughout, Professor Sanjian has adopted Xačikyan's purely chronological pattern, disregarding geographical location and ranging through Armenian communities from Cilicia to the Black Sea coast and even the Crimea (p. 100). The translations have scrupulously followed the texts without attempting to rectify their awkwardness or ambiguities, and, given the extreme difficulty of the material, a remarkable level of accuracy has been maintained. To treat Professor Sanjian's book merely as a translation would, however, be an injustice. The erudition of his appendixes takes them well beyond those of Xačikyan's edition and puts Armenologists as well as scholars unfamiliar with Armenian in his debt.

The most serious concern for historians using Professor Sanjian's book will be their distance from the original text. As the author points out in his preface (p. xiii), Xačikyan published only a part of the existing colophons, for not all of which had he seen the manuscript, and from which he deleted certain passages as insignificant stereotypes. In his turn Professor Sanjian has limited himself to taking from Xačikyan's choices only those colophons, or portions thereof, that he considered "historical." Consequently, readers may at times feel hesitant in interpreting the highly selective and fragmentary evidence presented to them. Armenian specialists will certainly wish to read the originals for themselves, but the wider circle of Middle Eastern scholars should undoubtedly be grateful to Professor Sanjian for having made available and having elucidated this valuable and heretofore inaccessible material.

Columbia University

NINA G. GARSOÏAN

Africa

ISLAM IN AFRICA. By *J. Spencer Trimingham et al.* Edited by *James Kritzeck* and *William H. Lewis*. (New York: Van Nostrand-Reinhold Company. 1969. Pp. viii, 339. \$8.75.)

WHEN new fields of research are being opened or extended, collaborative volumes can be of great value. They sum up existing knowledge, provide valuable materials for instructional purposes, and clarify key questions for future research. This volume does none of these. The blame lies with the editors, who assembled a disparate group of contributors, imposed few guidelines or central questions, and accepted some poorly written articles. The proofreading was careless and the orthography eclectic.

The book starts with a section on "Historical Perspectives." J. Spencer Trimingham's discussion of the expansion of Islam is a condensation of theories he has elaborated in greater detail elsewhere. It suffers from a self-assurance that the evidence does not always warrant. Norbert Tapiéro's similar overview lacks the coherence of Trimingham's treatment and suffers from an ignorance of recent historical writing on Africa. Most surprising is his resuscitation of the totally discredited Hamitic theory. Several of the peoples cited, the Bahima and Masai, are neither Moslem nor Hamite (if such existed) and thus did not belong in this volume. Trimingham also uses the term "Hamite," but uses it with justifiable restraint to mean speakers of Afro-Asiatic languages.

The section on "Aspects of Islam" includes good articles by Humphrey Fisher on Moslem separatism in West Africa and by Hatim Amiji on Asian minorities in East Africa. There is also a carelessly written piece by Vincent Monteil on marabouts, which starts well but digresses into an anecdotal and uninformative discussion of fraud and superstition. The third and longest section is a series of regional and

national studies, which includes good chapters by Crawford Young on the Congo, Pierre Alexander on Cameroon, Mervyn Hiskett on Northern Nigeria, and Nehemia Levitzion on Ghana, Togo, and Dahomey.

Young's piece on the Moslems of the eastern Congo is a high light in contrast to the inadequacy of many of the other articles. Young is not an Islamicist or a historian, but he has read widely in Central and East African history, he writes clearly, and he does not wander off into irrelevancies. Furthermore, he focuses on key problems: the formation of the community in the nineteenth century, its survival in the face of Belgian hostility, and its role in recent history.

In general, it is the discussion of recent history that is most inadequate in this volume, though this seems to be the central concern of William Lewis. In a rather superficial article on "Nationalism and Modernism," Lewis briefly discusses Guinea, but he neither touches on the nature of the Moslem community there nor its relations to the Touré regime. Lewis also fails to note the strong correlation between Islam and radicalism, largely because he is trying to convince himself and us that Islam is a conservative force in Africa.

The radicalism of Moslem nations is only one of many questions that this book does not discuss. There is no systematic discussion of Moslem-African institutions or of Islam's role as an alternative path to modernity. And there is little discussion of Islam's efforts to transcend traditional kinship and political ties or of the relations of African Islam to the Middle East, which so strongly influence African international relations. One of the problems is that the editors see Islam in Africa as a "folk religion," and therefore seem to assume that it must be analyzed in different terms from other Moslem communities. Most black African writers on Islam would dispute this, and, in fact, it is surprising that none are contributors to this volume.

Islam has shaped the thought and action of much of Africa. It has received and will receive from others a fuller and more sensitive treatment than this book gives it.

University of Toronto

MARTIN A. KLEIN

BENIN AND THE EUROPEANS, 1485-1897. By *A. F. C. Ryder*. [Ibadan History Series.] ([New York:] Humanities Press. 1969. Pp. xi, 372. \$8.50.)

THE Benin scheme for the interdisciplinary study of the Edo kingdom was launched at the University of Ibadan in 1956. The present volume grew out of Dr. Ryder's several years of research in that connection with the archives of Portugal, England, Holland, and the Vatican. It makes no claim to be a general history of Benin, but rather is an account of Benin's commercial and political relations with the European powers over more than four centuries. This limitation of the topic is disappointing in one sense—many hoped the European archives would have far more data on the internal history of Benin than turned out to be the case. On the other hand, Ryder's painstaking research and careful presentation of Benin's contact with Europe is a welcome contribution, and it fits into a pattern of recent studies of West African trade and trading practices.

In this broader West African context, it becomes clear from Ryder's research why it is that the European records have so little to say about Benin's internal affairs. Benin was never oriented toward the seacoast, but toward the interior. As a matter of policy, the Obas were able to avoid mass export of slaves at most periods. For the whole pre-colonial period of maritime contact, cotton textiles and ivory were more important exports than were slaves, and Benin's foreign commerce was far more closely

integrated with nearby parts of Africa than with Europe. The Europeans carried away the cloth, but they mainly sold it elsewhere in Africa. The slave trade of the early sixteenth century was a coastal trade, with slaves from Benin sold again on the Gold Coast. Comparatively few of the ships calling at Benin ports came direct from Europe. Most were coasters based at São Thomé or on the Gold Coast. The general point that emerges is important: a major African state near the coast could remain a major African state through the period of maritime contact without having a great deal to do with Europeans.

University of Wisconsin, Madison

PHILIP D. CURTIN

NORTHWEST TANZANIA UNDER GERMAN AND BRITISH RULE:
COLONIAL POLICY AND TRIBAL POLITICS, 1889-1939. By *Ralph A. Austen*. (New Haven, Conn.: Yale University Press. 1968. Pp. x, 307, \$8.75.)

THE frontiers of African history are being pushed back in a number of valuable ways, not the least of which is the emphasis on geographical entities smaller than the nation-state. The present book, which reached me only in 1970, approaches a relevant section of Tanzania longitudinally and shows how German and British attitudes toward and techniques of colonial rule were applied therein. Using an extensive selection of documentation in several languages, the author's method is to divide the colonial period into chronological segments and, within each, to follow a chapter on territorial practices with separate chapters on the Mwanza (Sukuma) and Bukoba (Haya) provinces of northwestern Tanzania. Within each chapter there is an approximately standard functional breakdown, with brief sections on such topics as economic development and educational change.

This is a probably natural outgrowth of the book's original presentation as a doctoral dissertation, but it does not make for inspired or particularly readable micro-history. Nor does Austen contribute anything of particular significance to the important and ongoing debate on indirect rule. After reading his book, it is possible to conclude that indirect rule, as a concept, was a theoretical rather than a practical construct at the local level. Austen's material seems to demonstrate that, despite its forceful imposition on Tanganyika during the 1920's, the methods of centralized provincial and district administration made mockery of territorial rhetoric and direction. In this way, the British mode of governance paralleled the German. These are among the implications of this detailed, very competent, and exceedingly well-controlled study, but the author's focus is never very sharp or truly comparative. Much as we are able to learn of administrative minutiae, this is a monograph that fails to make the kind of larger methodological or substantive contribution that is the true rationale for microhistory.

In addition to a surprising sloppiness in the notes and some inadequately grounded generalizations in the early parts of the book, there is another criticism that should be made. In his introduction the author promises to emphasize mainly the African side of "the very unbalanced equation" of colonialism. But the few Africans who do appear individually or en masse in the present book have a cardboard quality. Whites, especially a few administrators, seem—perhaps only because the author has adhered very closely to his Eurocentric documents—better represented and understood. Like so many books of its genre, this one exudes the air of the metropolitan outpost. It is hardly history from below.

Massachusetts Institute of Technology

ROBERT I. ROTBERG

Asia and the East

INDIANS IN MALAYA: SOME ASPECTS OF THEIR IMMIGRATION AND SETTLEMENT (1786-1957). By *Kernal Singh Sandhu*. (New York: Cambridge University Press. 1969. Pp. xxiv, 345. \$18.50.)

INDIANS IN MALAYSIA AND SINGAPORE. By *Sinnappah Arasaratnam*. (New York: Oxford University Press for the Institute of Race Relations, London. 1970. Pp. xiii, 214. \$3.50.)

THE course of Indian immigration in modern times has roughly followed the contours of the British imperial expansion. In the context of the current controversy over admission of British passport-holding Asians from the countries of former British East Africa, the question has assumed more than academic significance. In Malaya the balancing role of the Indian minority (9 per cent) vis-à-vis the Malays (47 per cent) and the Chinese (42 per cent) in the events leading to the formation of Malaysia in 1963 and in the withdrawal of Singapore from the federation in 1965 as well as in the electoral politics of the region has been important and, at times, crucial. Despite the obvious importance of the larger subject of the "overseas Indians," there were few studies of a scholarly nature until the last decade, which witnessed a spate of writings on Indians in the West Indies, Fiji, East Africa, Great Britain, and elsewhere. There have appeared in the 1960's more than a half-dozen works on the Indian community in Malaya. Of these, the two books under review along with J. Norman Parmer's *Colonial Labour Policy and Administration* (1960) and Usha Mahajani's *The Role of Indian Minorities in Burma and Malaya* (1960) are certainly the most outstanding. Professor Sandhu's work is a revised version of his doctoral dissertation at the University of London; Professor Arasaratnam's contribution is one in a series of studies on interracial relations published by the Institute of Race Relations, London.

Both the authors have treated migrants from the Indian subcontinent as one group, without distinguishing between those of Indian and Pakistani origin. Professor Arasaratnam, himself a Malaysian of Ceylonese origin, has devoted some space to tensions between Indians and Ceylonese, pointing to the latter community's feelings of superiority based on an "almost middle-class white-collar" character. Despite their community affiliations (Professor Sandhu is of Indian origin), both the authors have demonstrated an exemplary objectivity, eschewing the easy temptation to glorify or vilify one or the other of the several ethnic groups in Malaysia and Singapore.

Professor Sandhu's book is descriptive; he traces the process of migration, the linguistic and regional orientation of the immigrants, and the patterns of their settlement and occupation; and he supports each subject with abundant statistical evidence and maps. Part III of his book dealing with the position of Indians over the decades in the economic, political, and administrative fields will for a long time remain a basic source to any student of Malaya's economic history. Professor Sandhu's research is thorough, drawn from primary materials in the archives of Malaysia, Singapore, India, and Great Britain. One would wish, however, that he had preferred an abbreviated form of titles to the acronyms for over 130 books and reports frequently cited in the footnotes.

Dr. Sandhu's study spans the period of British political presence in the Malay archipelago beginning from the occupation of Penang in 1786 to the advent of the Merdeka in 1957. On the other hand, despite the obvious focus on the race relations in recent times, Professor Arasaratnam has given a valuable historical perspective,

though he refers very sparingly to the cataclysmic interracial tensions and riots of the last decade. This is, in a sense, unfortunate and detracts seriously from the value of the two studies. After all, Malaya was long presented as the showpiece of a successful pluralistic society whose three segments—Malay, Chinese, and Indian—lived harmoniously in a common polity. Equally significant would have been a study of the impact of Indian foreign policy and of the reaction of the Indian minority to the Chinese invasion of India in 1962. The Singapore foreign minister's Indian origin and the preponderance of the Chinese community in that island state would have provided some interesting insights.

University of California, Los Angeles

D. R. SARDESAI

VIETNAM AND CHINA, 1938–1954. By *King C. Chen*. (Princeton, N. J.: Princeton University Press. 1969. Pp. xv, 436. \$12.50.)

GENEVA 1954: THE SETTLEMENT OF THE INDOCHINESE WAR. By *Robert R. Randle*. (Princeton, N. J.: Princeton University Press. 1969. Pp. xviii, 639. \$17.50.)

THESE books reflect the long time the authors spent in research, the careful examination of available sources, the concern for detail, and the effort to arrive at conclusions based on the evidence. Some of their viewpoints can be debated, but they cannot be ignored. They are not just conclusions founded on idle fancy or merely the results of an attempt to marshal evidence to support preconceived premises. Any future author who fails to list these books in his bibliography on the subjects covered will do so at his own intellectual risk.

Professor King C. Chen's study on Sino-Vietnamese relations from 1938 to 1954 is better from the Chinese than the Vietnamese approach. This point is well brought out in the early part of the bibliography, for his Chinese sources excel his Vietnamese. At the same time his search for Chinese Communist as well as Vietnamese Communist sources (when he was focusing on this aspect) did not lead to any academic bonanza. Ho Chi Minh, for instance, was quite capable of some frank and earthy expressions of thought (as Chen vividly points out), but the Vietnamese leader never published a true account of his innermost thoughts leading to his major decisions. Nor is it likely that he kept a diary or left an account behind him.

The thrust of Professor Chen's book is on the rise of Ho Chi Minh to leadership and power amid Communist and non-Communist Vietnamese contenders as well as on the attitudes and policies of Nationalist China and then Communist China toward him up through the Geneva settlement of 1954. The thrust is not on why and how the United States became involved in the Second Indochinese War. The author deals briefly in a tantalizing way with the romance of the Office of Strategic Services (OSS) with Ho Chi Minh in late 1944 and 1945, but he calls attention to the limited available sources on the subject. It is indeed a pity that this phase of American-Vietnamese relations over a quarter of a century ago is still left essentially to conjecture. Another phase that receives a little attention is the policy of the United States in the First Indochinese War, 1946–54.

It is in Professor Chen's analysis of the relations of a weak Nationalist China—one might even say various factions of Nationalist China—and of a strong Communist China with Ho Chi Minh and his colleagues that the book makes its greatest contributions. Chen pointedly observes on page 331: "While Chinese Nationalist policies differed remarkably from those of the Communists, the pattern of the

relationship showed no major change from that of the past." The author's analysis of the fundamental elements in the evolution of Sino-Vietnamese relations is excellent although he strays in some other aspects of his epilogue from the basic theme of the book into the controversies of the present.

Robert F. Randle has written the best study to date of the Geneva Conference and Accords of 1954. Even when American and foreign archives are fully open to the scholar (if they ever are), it is doubtful that they will make this book basically obsolete. Considerable emphasis in the analysis is placed on the legal aspects, and the layman may find himself disadvantaged before the lawyer. Indeed, the author indicates in the preface his own interests in practicing law and in teaching. Since many of the provisions of the Geneva Accords were purposely loosely drafted in order to attain the settlement of July 1954, considerable leeway was provided for legal interpretation and political controversy. The election provision was particularly a case in point.

Randle has conducted research in the Dulles papers at Princeton and has made use of the Dulles Oral History Collection. His interpretation of the American foreign policy leader in the Eisenhower administration will be challenged. "My study of Mr. Dulles," he writes on page ix, "has led me to conclude that accounts that picture the late Secretary of State as an inflexible exponent of his own, puritanically inspired views of world politics are probably inaccurate and grossly oversimplified." On the basis of the evidence, Randle believes that Dulles, at least in the Indochina crisis of 1954, was astute and acutely aware of political realities at home and abroad.

Although attention has been called in this review to Randle's stress on the legal aspects and on the role of Secretary Dulles, the organization of his study in three parts—the first dealing with the diplomacy of nonintervention prior to the Geneva Conference, the second with the conference itself and international politics, and the third with the settlement focusing on analysis and implementation—reveals the broad dimensions and full scope of the book. Randle has given the specialist a volume that will test his mettle.

Readfield, Maine

RUSSELL H. FIFIELD

CHINESE GOVERNMENT IN MING TIMES: SEVEN STUDIES. By *Tilemann Grimm et al.* Edited by *Charles O. Hucker*. [Studies in Oriental Culture, Number 2.] (New York: Columbia University Press. 1969. Pp. xi, 285. \$12.00.)

THE seven papers collected in this well-edited volume are the product of a research conference held in August 1965 on Ming government. The work as a whole presents the most current and important research results regarding the Ming state. It is certainly a welcome contribution to this country's growing scholarship on the Ming dynasty in particular and to the knowledge of traditional Chinese governmental institutions in general.

In the erudite opening essay, Professor Lien-sheng Yang discusses the Ming local government in the context of the age-old problem of the guiding philosophy of empire: the balance between feudal system (decentralization) and prefectural system (centralization) of political power. The conclusion is that local governments under the later dynasties of Imperial China, particularly since the Sung, became extremely overcentralized. In the case of the Ming, powers in the local government were also excessively divided among officials at various levels and of various capacities. Professor Yang's paper presents a cluster of themes and problems that can be followed through the rest of the volume.

In the second essay, Professor Romeyn Taylor explores the Yüan origins of the *wei-so* system. He finds that the founder of the Ming dynasty Chu Yüan-chang fashioned his military system after the elite *wei* of Yüan and created a military elite of hereditary office- and title-holders from among his soldier companions. While I admire this illuminating study, I wonder if the comparative remarks on the military systems of Western Wei, Sui, early T'ang, and Sung could have been improved by consulting the works of Ku Chi-kuang (1962), T'ang Chang-ju (1962), Ts'en Chung-mien (1957), Chang Yin-lin (1937), Sogabe Shizuo (1937, 1942), and Tanikawa Michio (1963). These works offer fresh perspectives on the problem of the basic nature and historical origins of the *wei-so* system. Perhaps Hsieh Yü-ts'ai's work (1940) should also be mentioned here because it addresses itself partly to the same problem as the work of Professor Taylor.

Professor Jung-pang Lo's paper is a pioneering study of policy formulation and decision-making in Ming China. He points out that in traditional China in general and in the Ming period in particular the participation and deliberation procedure of decision-making on peace and war was not substantially different from that followed by modern states, and that the decisions were seldom impulsive acts of the ruler or any other single individual, as some scholars have assumed. This view makes clearer both the actual working and basic nature of traditional Chinese government as well as the role of the emperor in it. Its ideological basis can be readily found in political philosophies of the classical period as expressed in the *Shang-shu* and the *Li-chi* and by Confucius, Mo Tzu, Mencius, and others and even more in the political philosophies of the later imperial period as expressed by Ssu-ma Kuang (1019-86), Teng Mu (1247-1306), and later by Huang Tsung-hsi (1610-95), to name only a few.

Professor Raymond Huang's stimulating article reviews the Ming fiscal institutions and investigates the causes of the financial crisis in the early seventeenth century. His meticulous research reveals that, contrary to general impression, during the period from 1570 to 1600 the imperial revenue neither increased nor decreased in any appreciable volume. But in the seventeenth century the level of economic activities had reached a higher plateau, and increase in tax revenue was an unavoidable consequence. While scholars generally consider that this late Ming tax surge caused a financial crisis and led to the rise of peasant rebellions, Huang, on the contrary, holds that the failure of the Ming court in this period was not due to overtaxation, but rather largely to the breakdown of the governmental machinery to enforce its own tax laws and to curb local abuses. Viewed together with other similar interpretations, this illuminates our understanding of the complicated problem of the fall of the Ming dynasty. As a whole, Professor Huang's work is a major contribution to the knowledge of Ming financial history, and it has opened new avenues for further research on the subject. I wonder, however, how much effort could have been saved by the use of the *Wan-li k'uai-chi lu* (rev. ed. [1582]). Being the fiscal record of the Ming down to the Wan-li period, it certainly is a most essential work for this kind of study. Research on this invaluable, rare source will definitely produce fresh results to widen the horizon of our knowledge of Ming fiscal structure.

Professor Tilemann Grimm's splendid study concentrates on the education intendancy, which supervised the school systems on the provincial level. His overall view is that the Chinese civil service examination had its formative period from the Sung (960-1279) to the early years of the Ming dynasty; it matured during the Ming period. Being a part of the system, the evolution of the education intendancy followed the same pattern. Specifically, his examination of the changing emphasis on the in-

tendants' evaluation method led him to conclude that by later Ming times "bureaucratism" had fully triumphed over "intellectualism." Professor Grimm's meticulous research produces new insights into the understanding of the educational structure and its relationship with the government and society in late Imperial China. A few significant points need correction, however. Civil service examinations were first instituted not by the Han Emperor Wu, but rather by his grandfather Emperor Wen in 165 B.C. (*Han-shu* 4, 49; also Teng Ssu-yü, *Chung-kuo k'ao-shih chih-tu shih* [1936]). The post of education intendant or an office of this sort emerged earlier than 1103 or 1105, probably in 1100 (*Sung-shih* 157; *Sung hui-yao*, esp. *Ch'ung-fu* 2; *Wen-hsien t'ung-k'ao* 46, for examples). The system continued in the Southern Sung, as clearly recorded in the *Sung-hui-yao* (*Ch'ung-fu* 2, *Chih-kuan* 47, for examples), *Tsun-Po-t'ang chi* 6, and other sources. Various sources indicate that it attained a regular place in China's governmental structure in the Chin (1115-1234) and Yüan (1260-1368) periods.

Professor John Meskill's research on the *Shu-yüan* (academy) institution and its entanglements with politics has made an important contribution to the understanding of the close connection between scholars and politics in Ming China, and it provides a standpoint of such analysis different from the one based on civil service examination and the public education system, both of which are discussed in two other papers in the book (Grimm, Parsons). The political experience of the *Shu-yüan* in Ming society, as has been carefully detailed by Meskill, clearly suggests how independent educational institutions, or any organizations for that matter, gravitated toward political activities in a government-centered society.

In the last paper, Professor James B. Parsons' painstaking analysis of the geographical and social origins and the career patterns of the Ming bureaucracy points to a variety of significant problems. He holds that the fall of the dynasty was attended by an intensified and general bureaucratic chaos, where officials were replaced with bewildering rapidity, a view that significantly complements the one advanced by Professor Huang as mentioned above. Professor Parsons maintains that the Ming bureaucracy was erected on a broadly based foundation and that there simply did not exist a narrowly based political elite that enjoyed an extended grasp on power. This provides a considerable contrast to the earlier periods of Chinese history when great clans held power throughout a long period. Here it is significant to point out that the dividing line of these two situations occurred in the Sung period. A study on the social origins of T'ang-Northern Sung officials by Sun Kuo-tung in 1959 (*Hsin-ya hsüeh-pao*, 4:1) has well documented this point. Professor Parsons' study reveals that the key Ming centers of political power were the lower Yangtze Valley (Nan-chihli, Kiangsi, and Chekiang), the upper north China plain (Pei-chihli), and the upper southern coast (Fukien). Thus, in general, Ming political power coincided with economic strength and cultural dominance. Professor Parsons' conclusion points to the overwhelming dominance of South China in both the political and cultural realms. This fascinating subject has long attracted the attention of scholars. Old works such as those of Ting Wen-chiang, Chu Chün-i, and others and recent works such as those by Ho Yu-sen (1955, 1956), Shimizu Taiji (various), Fu I-ling (various), Edward Schafer (1954), Chang Chia-chü (1957), Aoyama Sadao (1962), and a great many others all have treated the various problems Professor Parsons has discussed or touched upon.

In his preface, which describes the background and progress of the conference of 1965 and briefly introduces the seven papers, Professor Charles O. Hucker, who edited

this volume, has marked this book as celebrating the six-hundredth anniversary of the founding of the Ming dynasty. There is certainly no better way to commemorate this significant event in Chinese history. The Ming period has, since the 1920's, attracted the research efforts of modern Chinese and Japanese scholars; witness extensive bibliographies of Ming studies by scholars such as Yamane Yukio, Hirotsume Hitoyo, Arita Kazuo, Pao Tsun-p'eng, and others. The past twenty years have seen an upsurge of scholarly interest in all aspects of Ming China, among Chinese, Japanese, and Westerners alike. The publication of these seven papers sums up, and in some cases opens, new frontiers of our knowledge of Ming China.

University of Michigan, Ann Arbor

CHUN-SHU CHANG

CHINA'S TURBULENT QUEST. By *Harold C. Hinton*. ([New York:] Macmillan Company. 1970. Pp. xi, 340. \$7.95.)

DESPITE the intellectual limitations of adopting a know-your-enemy perspective, Professor Hinton, a former analyst at the Institute for Defense Analyses, has written the best general study of contemporary Chinese foreign relations published to date. Taking as his subject the first twenty years of the People's Republic of China, Hinton confronts head-on the many difficult questions of interpretation and weaves a vast array of seemingly unrelated international events into compelling (yet often debatable) explanations. His book is well written and readily comprehensible, apparently intended principally for the general reader or university student.

Hinton's approach will prove disturbing for many readers, however. The author's self-image, as implied in his analysis, is that of the tough-minded, clear-sighted realist, a man who assumes that power is the overriding issue of international politics and that only the naive would concern themselves with questions of right and wrong. For Hinton, imperialism cannot be valued as good or bad but was "inevitable," and America's only mistakes in intervening in Vietnam were matters of faulty strategy. Objectivity is the author's claim for his own value position, while alternative views are castigated as biased or "sympathetic."

Significantly, it is the perceptual limitations of Hinton's own outlook that, to my mind, seem to make for some of the more serious weaknesses in an otherwise competent analysis. For example, having acknowledged significant Chinese accomplishments over the years, Hinton nonetheless rejects out of hand the political ability of Mao Tse-tung and the practicability of his ideas: "Mao Tse-tung, whose 'thought' is a highly personal and Chinese reading of an imperfectly understood and intrinsically defective philosophy, Marxism-Leninism, cannot be said to have understood how modernization could be achieved in China." Likewise, the author's interpretation of the Cultural Revolution fails to go beyond an analysis of the power struggle to capture the motive force of that cataclysmic event produced by differences having to do with fundamental value questions.

Other, less significant shortcomings in the study that should be mentioned include: the complete lack of reference to checkable sources (footnotes were presumably omitted in the hope of winning a wider readership); a rather heavy repetitiveness in the second half of the book; and, finally, a faulty map appearing on the inside covers—twenty years after the establishment of the Republic, is it too much to expect a map of China that includes Hainan Island (taken in 1950!) and that accurately labels and delineates the major administrative units of the country?

In conclusion, Professor Hinton's study, despite its shortcomings, constitutes a

major contribution to a field of study still very much in the early stages of development. Students of contemporary Chinese foreign relations should find particularly useful the first half of his book, which describes in detail the history of Peking's international relations through 1968.

University of Denver

PETER VAN NESS

NISHI AMANE AND MODERN JAPANESE THOUGHT. By *Thomas R. H. Havens*. (Princeton, N. J.: Princeton University Press. 1970. Pp. viii, 253. \$8.50.)

This study of Nishi Amane (1829-97), a member of the intellectuals of the enlightenment period of the 1870's, is a fascinating analysis of his rejection of his own traditional ideology and acceptance of Western social theory. He served in both the old feudal government, which sent him to the Netherlands for two years to study economics, law, and political science, and, after 1870, in the war department of the new imperial government. He considered his primary task to be to interpret Western thought to the people and thus to enlighten them into giving up their old feudal habits.

Following a survey of the intellectual milieu of feudal Japan and the events that led Nishi to study Western languages and culture, the main part of this book is an analysis of three of his most important works. In *Hyakugaku Renkan*, a sort of encyclopedia reflecting the positivism of Auguste Comte and the inductive logic of John Stuart Mill, Nishi attempted to demonstrate the integral harmony of knowledge and that philosophy was the most important of all the "sciences." The manuscript remained unknown until 1932 and was first published in 1945. The second work, *Hyakuichi Shinron*, published in 1874, was a refutation of the Sung philosophical concept that politics and morality were identical. The third work, which appeared in 1875, proposed a new ethics for society. In it he advocated the "three human treasures" of health, knowledge, and wealth as a substitute for the feudal characteristics of loyalty, subservience, and frugality.

Professor Havens concludes his book with a discussion of Nishi's attitudes toward current problems and his contributions as a bureaucrat to the organization of the new imperial army, to the morale of the conscripts, and to an awakened loyalty among the officers of the armed services. Through the extensive use of basic source materials, the author has given us important new insights into the Japanese mind and intellectual tradition and has made a valuable contribution to the history of thought.

Columbia University

HUGH BORTON

THE NEW GENERATION IN MEIJI JAPAN: PROBLEMS OF CULTURAL IDENTITY, 1885-1895. By *Kenneth B. Pyle*. (Stanford, Calif.: Stanford University Press. 1969. Pp. viii, 240. \$7.95.)

CONSIDERING the great interest Western historians have in intellectual history, it is very surprising how little has been published in Western languages on that subject with regard to modern Japan—exclusive, that is, of literary studies. A certain amount of material has appeared for pre-Meiji and early Meiji, but until recently there has been almost nothing exploring the rich variety of intellectual activity to be found in the late Meiji, Taishō and Shōwa periods, from around 1890 on. Fortunately, during

the last few years, a number of younger scholars have undertaken investigations that make a beginning toward filling this important, long-felt gap in our understanding of modern Japan.

Among the first of these works to be published is the present book, which seeks to examine the impact of the rapid, breath-taking changes of Meiji Japan upon the generation of intellectuals produced by the newly established schools of higher education. The book focuses upon the early thought of the long-lived Tokutomi Sohō, with lengthy expositions of the contrasting ideas of Kuga Katsunan, Miyake Setsurei, and Shiga Shigetaka. It was an era much like the present, when the wisdom and experience of parents seemed to have little relevance to the concerns of youth. "A youngster of sixteen," wrote Tokutomi, "confronts problems of life that his ancestors, even his elders today, never imagined." The search for means of coping with the inundation of Western ways and ideas led ultimately to an attempt to isolate those unique elements in Japan's heritage that could be used to preserve both Japanese pride and Japanese identity. Sad to say, certain of the concepts formulated by these young idealists came shortly thereafter to provide intellectual underpinning for an expansionist ideology and in a mutated, virulent form were embodied in that ultranationalist document of the 1930's, the *Kokutai no hongi*.

Professor Pyle's book sets a very high standard for those to come in this field. It is well organized and presents its themes with clarity. The ideas discussed are handled with sympathy, sensitivity, and good judgment. There are a generous number of apposite, good-sized quotations, each a sharp and well-turned translation.

City College of New York

ARTHUR E. TIEDEMANN

CHRISTIAN CONVERTS AND SOCIAL PROTEST IN MEIJI JAPAN. By
Irwin Scheiner. (Berkeley and Los Angeles: University of California Press.
1970. Pp. x, 268. \$6.50.)

Dr. Irwin Scheiner has produced a much-needed work that will help fill two gaps in the English-language literature on modern Japanese history: first, a relative lack of scholarly monographs on the important subject of Christianity and modern Japanese society; second, a similar lack on the subject of the inner struggle of intellectuals in the rapidly changing Meiji society. With the recent publication of studies by Tatsuo Arima, Kenneth B. Pyle, and, now, Irwin Scheiner, it may be said that the lacuna in our understanding of the psychological price of Japan's modernization has begun to be filled.

Scheiner's monograph is a study of samurai-turned-Christians in Meiji society, with emphasis on the Kumamoto band and Niiijima Jō. The author concludes that through Christianity many *déclassé* samurai "attempted to revive their status in society by finding a doctrine that offered a meaningful path to power"; that in so doing, however, they "accepted a transcendent authority that brought them into conflict with society about them." The first half of this thesis, of course, is the status-anxiety hypothesis of group motivation, but it should be pointed out that Scheiner offers this theory as one of a number of explanations for the conversion experience. The second half of the thesis is amplified mainly in the lengthy final chapter. This chapter cannot be taken, however, as an outgrowth of the preceding chapters; furthermore, it is concerned largely with social retreat rather than social protest. Perhaps the best part of the study consists of the two chapters devoted to Niiijima; both are illuminating. Given the nature of the work, however, Uchimura Kanzō should have been assigned

at least a chapter. As an aside, one unfortunately finds here a higher than average incidence of typographical errors.

University of Florida

RICHARD T. CHANG

FOLK ORIGINS OF INDIAN ART. By *Curt Maury*. (New York: Columbia University Press. 1969. Pp. 245. \$27.50.)

THE main virtue of this work lies in the author's collection of data from the "unfamiliar back country of Central India." Scholarly accounts of Hinduism, he believes, have relied more on "canonical lore" and have neglected the "living tradition." Thus they lose sight of its "actual exercise." Maury is impressed with the recent "objective assessments" of anthropologists with whom he shares the concept of "the fold roots of Hinduism." In fact, their findings support his "contentions" and "corroborate his conclusions."

His work concentrates on "iconographic, mythological and linguistic evidence, endeavors to provide a fuller exploration of the non-Vedic/non-Aryan components of Hinduism, to probe the import of indigenous India's religious heritage, and to further a more discerning apprehension of its experiential and notional premises." His main objective is to demonstrate how a "perspective of existence, immemorial in its inception, continues as the dominant influence in India's religious and secular life to this day."

Maury accomplishes his task in fourteen chapters while overemphasizing the roles of *śakti*, the serpent and lotus. The preservation of this heritage has made India's Great Tradition "a truly great tradition," he concludes and demonstrates "the universality of India's artistic accomplishment and its underlying religious inspiration."

The author merely extends Zimmer's imaginative approach to myths and Hinduism by adding Jungian psychology. Coomaraswamy's analysis of diverse elements in Indian art and religion contributed to a comprehensive understanding of the total culture, while Maury wants to isolate these diverse elements in order to emphasize the indigenous. He further betrays a Marxian bias when he attempts to find social conflict where perhaps it does not really exist and when he assigns a predominant role to women in the first phase of human history. Unlike Zimmer, Maury fails to appreciate the imaginative values he tends to identify with literal realities. Unlike Coomaraswamy, he unduly emphasizes universalism but fails to present a comprehensive picture of Hinduism.

Maury firmly believes that "art is the truest mirror of religion as an emotional actuality." This is maintained primarily by those who are more interested in using art to advocate some theory in a different field, Hinduism in this case. Hence Maury does not deal with art as such and fails in his main purpose. He never distinguishes the characteristic features of folk art (*deśhī*) from those of *mārga*. Consequently his work is not on folk art but rather on the folk origins of certain religious themes and iconography.

Whereas the author extols the virtues of field work, his reproductions (all the author's originals) are only in permanent materials. Folk art, however, exists in abundance in perishable materials. Had he considered the latter it might have helped to make the distinction between folk and great art. One wonders if the author purposely excluded them because they would upset his well-formulated theory! If the

author were not so much interested in solving so many problems and drawing so many conclusions his work would be of greater value.

Maury makes no reference to European anthropologists, especially Claude Lévi-Strauss. He shows no familiarity with recent authoritative literature on Tantrism nor with recent interpretations of India as viewed from "below." He does not adequately discuss society and polity nor does he clarify use of the terms Aryan and Hindu.

Free of misprints, the book is well organized. But, according to our art historian Professor Prithwish Neogy (with whom I discussed the book), the author is inaccurate in identifying Vishnu of figure 15 with Balaji; figures 171 and 172 both represent *nāgas*, not *nāginis*; plate 28 shows a male-female pair, not two *nāginis*. The use of italics and diacritical marks is inconsistent and the bibliography insufficient. The layout is inadequate and plates are difficult to locate, being scattered without proper indicators. The type used is too small and the printing too compressed. Though writing on folk art, his sentences read more like technical statements.

Despite these deficiencies, Dr. Maury has brought to bear a new perspective on the study of Hinduism, and his book is strongly recommended to the scholar.

University of Hawaii

JAGDISH P. SHARMA

IDEAS IN HISTORY: PROCEEDINGS OF A SEMINAR ON IDEAS MOTIVATING SOCIAL AND RELIGIOUS MOVEMENTS AND POLITICAL AND ECONOMIC POLICIES DURING THE EIGHTEENTH AND NINETEENTH CENTURIES IN INDIA. Edited by *Bisheshwar Prasad*. (New York: Asia Publishing House. 1968. Pp. xv, 351. \$7.25.)

HERE are the results of an advanced seminar convened at the University of Delhi in 1964. In a brief comment one cannot begin to name the twenty-five contributors and their papers (including a sophisticated summary of the discussion by Romila Thapar), let alone the entire fifty-six participants—many of whom are well known personally and through their writings to Western scholars concerned with things Indian. But all of these essays are interesting and some of them quite distinguished.

The editor set the themes in his address of welcome: "The Seminar will take cognisance of the ideas and institutions which were prevalent in the eighteenth century in India before it was brought into physical contact with the new European civilisation and its institutions, as well as the ideas and concepts which were peculiar to the new Western society, and analyse their interaction." This, then, was not a discussion of intellectual history, but rather an exploration of ideological causation in modern Indian history. The conference was divided into two major parts—ideas relating to social and religious motivation and ideas relating to political and economic motivation. Some of the papers, such as that of S. A. A. Rizvi ("Ideological Background of the Wahhabi Movement in India in the 18th and 19th Centuries") are quite specific, while some like that of Sabyasachi Bhattacharya ("Laissez Faire in India") take a broader perspective.

These essays are stimulating and graceful, although the scholar in the field will find himself familiar with most of the notions expressed. It might have helped to abandon the tired formula of British impact and Indian response for some fresher model. In her summary Romila Thapar suggests at one point a regional approach; indeed, recent investigation has shown this to be a useful model, although its utility is threatened by modishness and muddy concepts of "regionalism." A conscious

emphasis on continuity rather than change might have produced more new interpretation. Satish Chandra ("The Maratha Polity and Its Agrarian Consequences") has indeed done this. One may observe incidentally that, since history cannot be repeated under controlled conditions, what-might-have-been is also a legitimate consideration of the historian.

A number of revisionist trends were suggested by the contributors. The eighteenth century was not the socially feeble period it is often taken to be. A clear-cut distinction between reformers and revivalists is not justified. R. S. Sharma tells us that the means employed by the early reformers were not those of rationalism and utilitarianism (logical persuasion) but those of the *Śāstras* (proving social reform to be consonant with sacred text). Other writers tell us that the ascendancy of *laissez-faire* doctrine in the nineteenth century was not complete by any means and that Muslim separatism was a communal but not a religious phenomenon. The economic thought of nationalist thinkers was inevitably colored by their social origins, but to group all nationalist polemicists in a class is a mistake. As was to be expected, more questions were raised than answered. For example, why did no enduring religious synthesis between Hinduism and Islam develop? Or, how did Indians conceive of membership in the British Commonwealth? I cite these particular historiographical trends and questions only to suggest the variety of material presented.

The editor has done a very commendable job considering the difficulty of the task. If, however, he could have persuaded all the contributors to document their work and if he had included an index, this volume might have been easier to use. The chief thing one really learns here is the state of the art of modern Indian historiography in 1964.

Los Angeles Valley College

MARK NAIDIS

REFORM AND REGENERATION IN BENGAL, 1774-1823. By *Amitabha Mukherjee*. (Calcutta: Rabindra Bharati University. 1968. Pp. xx, 392. Rs. 16.50.)

THIS is a useful, scholarly book that with a little more courage and imagination might have pioneered a new approach to a crucial period of modern Indian history. More courage would have permitted the author to abandon, as he seems on the point of doing, the stereotype of eighteenth-century Bengal society that is a product of both British and Indian historiography. More imagination would have led him to exploit to the fullest the sources in Bengali he has obviously examined with care. Instead he had constantly returned to the idea of a corrupt society that is purified by social reform, with the evidence of "corruption" being those facets of Indian society that seemed so displeasing to British evangelicals and utilitarians as well as to their spiritual analogues, the nineteenth-century Indian intellectuals.

Thus while he accepts Long's evaluation of the schools that were in existence in Bengal, he is too sophisticated a scholar not to see that there must have been more vitality in the system than was apparent to Long, who, while deeply sympathetic, only saw that the schools did not fulfill the functions of an educational system as he understood it. The same is true of the whole range of customs and practices that were so denigrated at the time—*kulinism*, *sati*, hook swinging, and the rest. Mukherjee, more than any other writer on these familiar themes, seems to be on the verge of insisting they must be understood from within the context of the society, but he unhappily returns to the received positions.

One also wishes he had not tried to do so much, that he had explored more fully,

for example, the significance of the numerous schools that were started in Calcutta and its environs in the late eighteenth century for teaching English. Some of these were started by Indians, others by Englishmen in straightened circumstances who saw there was a market for their product. His chapter on Ram Mohan Roy contains many shrewd insights into both Roy's own career and that of the intellectual life of the time, particularly in his references to the membership of the Atmiya Sabha, but there is little elaboration and explanation. Mukherjee writes well, and one can hope that he will expand some of the ideas he has put forward in this book.

Duke University

AINSLIE T. EMBREE

NEW INDIA, 1885: BRITISH OFFICIAL POLICY AND THE EMERGENCE OF THE INDIAN NATIONAL CONGRESS. By *Briton Martin, Jr.* [Publications of the Center for South and Southeast Asia Studies.] (Berkeley and Los Angeles: University of California Press. 1969. Pp. xii, 365. \$10.00.)

IN his *New India, 1885* Briton Martin, Jr. has produced a solid piece of scholarship on the interplay between British policy and the beginnings of Indian nationalist sentiment. Based upon a wide range of source material, Dr. Martin's study reveals dramatically the constraints under which both ruler and ruled operate in a colonial situation.

Martin's contribution is a significant one. His analysis of the interactions surrounding the beginnings of the Indian National Congress is a considerable addition to the study of modern Indian history. Yet, one must seriously question whether or not a study of this type has utility in making the Indian nationalist movement more comprehensible.

Martin argues that the emergence of an Indian national organization was not inevitable, that such an organization evolved from what were then potentially resolvable conflicts between English-educated Indian elites and British imperialists. What emerges from his discussion, however, is exactly the opposite conclusion—that the convolutions of a British official policy that on the one hand spawned and encouraged a new class of educated Indians and that on the other hand rejected this class and its demands as inconsequential could not fail to produce the kinds of conflict that paved the way for the emergence of the Indian National Congress.

The basis of the contradiction in this study and in studies of this type lies in the preoccupation with educated Indian opinion and its response to British policy. While there is no dispute that this new class provided much of the dynamic for the Indian nationalist movement, explanation solely in terms of these actors produces what is, at best, a truncated understanding of history. As so narrowly drawn by Dr. Martin, the events of 1885 offer little or no aid in understanding the subsequent history of the Indian nationalist movement.

Dr. Martin's study really serves to point out how little we actually know about the Indian nationalist movement—at any stage—but particularly during its formative period. It is ironical, indeed, that, because our attention remains locked on the English-educated Indian, the whole discussion of the nationalist movement in India seems to reinforce the British imperialist argument that this whole thing was nothing more, to quote Dr. Martin's summary of that argument, than the posturings of "an uninfluential, meddlesome minority consisting largely of 'Bengali Baboos.'"

University of Virginia

GERALD A. HEEGER

SOUS-DÉVELOPPEMENT ET UTOPIE AU SIAM: LE PROGRAMME DE RÉFORMES PRÉSENTÉ EN 1933 PAR PRIDI PHANOMYONG. By *Pierre Fistié*. [Maison des Sciences de l'Homme. Matériaux pour l'étude de l'Extrême-Orient moderne et contemporain, Travaux, Number 5.] (Paris: Mouton & Co. 1969. Pp. 254. 39 fr.)

IN March of 1933 a young minister-without-portfolio in the revolutionary government of Thailand submitted to his colleagues a "National Economic Program" intended to fulfill the ideals of the "revolution" of 1932. Pridi Phanomyong, its author, was accused of being a communist and forced from the government into temporary exile, and liberal civilian influence on the government was ended as the military assumed control. Pierre Fistié, of the Centre d'Étude des Relations Internationales in Paris, describes in this short volume the conditions against which the program was directed, the influences that shaped it, and the reception it was accorded. He appends a translation of the program (from Landon's English version), together with King Prajadhipok's commentary upon it (translated by Saveng Phinith).

Much in this volume will be familiar to readers of Kenneth Landon's *Siam in Transition* (London, 1939; reprinted New York, 1969), although this is the first publication of Prajadhipok's commentary in any Western language. The early chapters on "Le contexte économique, social et politique" are cursory and based upon extremely limited sources. Certainly Fistié's strongest section is his tracing of Pridi's ideas back to courses given at the Sorbonne by Auguste Deschamps and Charles Gide when Pridi studied there in the mid-1920's and his assessment of their "realism" and unfeasibility. His most serious deficiency is his total reliance upon Western-language sources. Much that cannot be ignored has been written recently in Thai on this subject. Fistié's work begins to fill a serious void in the material on Thailand available to the French public, and at its best it contributes to the study of a subject seriously neglected in academic circles. Fistié begins the important task of relating Pridi to the Paris in which he studied: it remains to set him more accurately in the complex texture of Thai politics and society in which he long played such an important role.

Cornell University

DAVID K. WYATT

THE POLITICS OF FORMOSAN NATIONALISM. By *Douglas Mendel*. (Berkeley and Los Angeles: University of California Press. 1970. Pp. 315. \$7.95.)

FOR more than two decades the political jurisdiction of the National government of China has been confined to the single province of Formosa. During this time one of the thorniest and most controversial issues has been the status of the Formosan majority on the island. In his political analysis, Professor Mendel has recorded what serves essentially as a documentary record of the developing ideology of Formosan nationalism. Through interviews in Formosa, as well as among Formosans abroad who were in a position to talk more freely, and through the written record, he has reported a broad spectrum of attitudes and responses among native Formosans that reveal a cohesive sentiment. The period of his investigation centers on the years 1957 and 1961-64, although material was acquired through 1968. Professor Mendel's greatest limitation, which he recognizes, is the fact that it was impossible to employ a systematic method in polling public opinion in Formosa.

After a brief account of the historical background that fomented Formosan discontent with the National government, Mendel considers the cultural, economic, political,

and military development of the island. He observes that in each case the Formosans feel that they have not fully shared in any benefits that might accrue from these efforts. Education, they declare, is used for indoctrination to emphasize a Chinese consciousness; certain Formosan industries have been neglected, and unemployment continues to be high; the people have little voice in Nationalist policies, and elections are meaningless; and "liberation" of the mainland to the Formosans means only departure from their homeland. The complaints are not new to those familiar with the Formosan scene, but Mendel has attempted to make a fair and full presentation for the record. The latter chapters of the book, concerning Formosan politics abroad and the views of other countries, official and private, are well executed and can be praised for their balance.

Regarding the future of Formosan nationalism, Professor Mendel ends on a pessimistic note. He discounts the much-rumored "deal" between the post-Chiang Kai-shek government and the China mainland to reunite, the evolutionary growth of the "movement" that would overshadow Chinese dominance, or United Nations leadership to enforce its supremacy. While he asserts that "the people and territory of Formosa are fully capable of supporting a viable state," he believes that only revolution and foreign assistance could assure Formosan dominance of the island. This is unlikely because the revolutionary potential of Formosan nationalism is very low.

Purdue University

LEONARD H. D. GORDON

Americas

THE WORLD THE SLAVEHOLDERS MADE: TWO ESSAYS IN INTERPRETATION. By *Eugene D. Genovese*. (New York: Pantheon Books. 1969. Pp. xii, 274. \$5.95.)

INDUSTRIAL SLAVERY IN THE OLD SOUTH. By *Robert S. Starobin*. (New York: Oxford University Press. 1970. Pp. xiii, 320. \$7.95.)

RACE RELATIONS IN VIRGINIA & MISCEGENATION IN THE SOUTH, 1776-1860. By *James Hugo Johnston*. Foreword by *Winthrop Jordan*. (Amherst: University of Massachusetts Press. 1970. Pp. xii, 362. \$10.00.)

In recent years many valuable studies of slavery and the Old South have appeared, and these three books, each in its own very particular way, are significant additions to the growing total.

Professor Genovese's contribution is basically an extension of his earlier *The Political Economy of Slavery*, and it is by far the most theoretical and interpretive of the three volumes. The author examines modern slavery in the Americas within a Marxian (or Genovesian-Marxian) framework, which inevitably increases the difficulty of communicating with and convincing a predominantly non-Marxian audience. Nevertheless this book is a learned, skillful attempt to master some very complex historical problems. Genovese does not really try to convert the reader to any elaborate historical schema, but rather he attempts to prod and provoke, to agitate and analyse, to broaden and deepen the study of modern slavery. In two essays of a little over one hundred pages each he succeeds well enough to make this book required reading for every serious scholar in the field.

The first essay is a comparative study of various slave societies of the Americas with emphasis on their European backgrounds—from the primarily "bourgeois" system of the British in the Caribbean to the predominantly "patriarchal," "paternalistic," almost

“seigneurial” but definitely not “feudal” system of the Old South. He places even more stress, however, on the master-slave relationship and class structure in the New World.

The final essay concentrates on the Old South and the writer Genovese considers its most significant spokesman, George Fitzhugh. Although Genovese rates the Old South as one of the most “prebourgeois” or “nonbourgeois” slave societies, he also grants that it was significantly influenced by dynamic capitalism. I prefer to reverse his formula and describe the Old South as a capitalistic society with paternalistic overtones, but this particular debate still has a long history ahead of it. Though he is certain to be so labeled, Genovese is no latter-day doughface, and, while he applauds some of Fitzhugh’s attacks on the menace of world capitalism, his detailed evaluation of his writings is far from uncritical.

Other aspects of this volume are open to question—the emphasis on class rather than race in the development of slavery and the use of the term “slaveholders” to designate a bewilderingly diverse jumble of individualists, for example—but, overall, Genovese has produced a well-written, massively researched book that is always impressive, sometimes convincing, and never dull.

Professor Starobin’s *Industrial Slavery* began as a dissertation, and compared to Genovese’s study it is a much more conventional historical monograph, which minutely examines the Old South’s use of slave labor in industry and transportation. Beginning as early as the 1790’s but especially in the last two decades before the Civil War, slaves labored in a great variety of industrial enterprises—textile manufacturing, mining, tobacco processing, tanning, milling, sugar refining, ginning and pressing cotton, manufacturing finished products in modern plants like the Tredegar Iron Works, constructing and operating railroads, turnpikes, ferries, and other transportation facilities, producing hemp bagging and rope, fishing, extracting turpentine, general lumbering, and practically every other industrial activity however nascent or minor. By the 1850’s five per cent of the slave population was employed in industry, which was a small but significant facet of the Southern economy.

Planters, not merchants and bankers, were the main supporters of ante-bellum industry, and more than half of the black industrial workers were in rural areas. Like plantation field hands, they lived at the subsistence level under rigid control and, when necessary, ruthless repression. Sometimes integrated with white workers, often hired out by planter-owners, occasionally resisting but usually adapting to the inevitable, the industrial slave was still very much a slave. Industrial slavery was viable and profitable and so efficient that it was incorporated into many state and even some federal enterprises.

Starobin, assistant professor of history at the University of Wisconsin at Madison, has handled a complex subject with great skill, and only in the last chapter does he tend to carry his interpretation too far by stressing the connection between industrial slavery and the secession movement. Overall, this is an excellent study, relentlessly researched, clearly written, effectively illustrated, thoroughly annotated, and further reinforced with a useful bibliographical essay. About the only thing lacking is a paperback edition to give this work the wider audience it deserves.

The last volume is essentially a dissertation completed at the University of Chicago in 1937 by James Hugo Johnston, who is now retired after many years of service as a teacher and administrator at Virginia State College. His book still retains some of the stiffness and awkwardness characteristic of most dissertations, but it also contains a rich lode of original information about the black and white folk of the Old South.

Professor Johnston lets long-dead Southerners speak for themselves through court

records, petitions to governors and legislatures, official and private correspondence, and many other primary and secondary sources. Most of these records are from Virginia, but, whatever their origin, they radiate a flesh-and-blood realism that allows for little debate.

Ante-bellum Southerners lived in a complex, often contradictory social milieu; few were heroes and few were villains. Most were ordinary mortals caught in a way of life contorted and corrupted by racism and slavery. Law and legend to the contrary notwithstanding, the races mixed extensively. This usually occurred through the union of a white master and a female slave, but it was not unknown for a white woman to bear a mulatto child. The mulatto, slave and especially free, experienced a peculiar and often precarious existence between two worlds that both blended and stayed apart; and a few hearty emancipationists, spurred on by sympathy for the slaves and fear of insurrection, struggled vainly for peaceful reform.

The author's main achievement is the painstaking research that has unearthed some of the raw realities of the Old South. Specialized scholars have benefitted from this research for decades; now at last a broader reading public with a real need to know can share the fruits of his labor.

University of Georgia

F. N. BONEY

INTERPRETING AMERICAN HISTORY: CONVERSATIONS WITH HISTORIANS. By *John A. Garraty*. ([New York:] Macmillan Company. 1970. Pp. xiii, I-367, II-334. \$10.95.)

THIS is a fascinating book, half pulpit, half pillory, providing a highly illicit blend of pleasure and sympathy to the reader who sees so many old friends taking the stand and abiding by the consequences. Can it be that the surprisingly low level of likenesses in the accompanying pointillist portraits reflects the unnatural strain to which Professor Garraty's cross-examinations subjected his victims? To be forced to compress one's life's findings into a capsule, to come down on one side or the other, to leave Mugwumpery behind in the study and become Mr. Standfast before the tape recorder—for most members of our profession this is a hard ordeal. Some, it is evident from these pages, embrace their fate with relish. Others can almost be seen to writhe on Professor Garraty's ineluctable pin. Yet others, one suspects, have had the worst scars of the confession process eliminated by Professor Garraty's ingenious editing before making their appearance in the court room.

Professor Garraty is very frank with his readers and describes the process of this new kind of book-making very clearly. It starts with a list of questions submitted to the victim in advance. Then comes the session with the tape recorder, a day of free-ranging discussion. From this is prepared a digested typescript that the historian can revise and amend. Thus the final report, while retaining the form of an interview, is at once more and less than that. The result is an acceptable compromise, particularly on first reading, which retains some of the vitality of a direct exchange while avoiding most of the untidiness and flabbiness of recorded conversation. But of course it is a method that suits some subjects a great deal better than others. Not all the interviews have color and personality. Not all the "characters" who come through most strongly are those with the most to say. Not all types of history lend themselves equally well to this Socratic, encapsulated treatment. The history of ideas enjoys a certain built-in advantage. Diplomatic history, with its tight dependence on the documents, or the kind of economic history that sees its problems in predominantly

statistical terms, are obviously going to feel, in their different ways, cramped or distorted. It is a tribute to Professor Garraty's editorial skills that the reader is so seldom conscious of these limitations.

The questions the editor puts to his experts differ, of course, according to the topic under review. But most interviews end with an invitation to assess the contribution or the impact of New Left interpretations. The responses vary in frankness or sympathy and in general are of less use to the reader than the other open-ended query sometimes put, about the "unanswered questions" in the subject's specialism. One cannot help feeling that the value and comprehensiveness of these conversations would have been enhanced if Professor Garraty had yielded to his initial impulse and included a New Left historian to speak directly for his school of thought—or indeed an Old Left spokesman; there is an almost unhealthy Marxist silence about the greatest capitalist society in human history.

It would be invidious to single out individual contributions from these twenty-nine "half-hours with the best historians," as our Victorian ancestors would undoubtedly have described this useful and entertaining anthology. But certain general reflections do come to mind as one watches this sequence of leading scholars testifying to their conclusions and convictions. The first is of the remarkable range of talent and interest that Professor Garraty has been able to assemble without in any way straining the principle of representativeness. Indeed, rather the reverse; colonial history might feel itself under-represented, with at most three spokesmen. This leaves some twenty-six historians to deal with the passage of less than two hundred years. Is there any other national history that can show a comparable intensity and quality of cultivation over any two hundred-year period? Perhaps only the *Pentekontaetia* and the French Revolution have thrown up a comparable wealth of diverse and fertilizing historical interpretations, and they have a vastly shorter time span. To have sustained such a high level of debate, such a continuing freshness and bite in the analysis of the past, over so long a period, is a striking demonstration of the vitality of American historiography and its persistent relevance to the problems of American culture.

At the same time a foreign observer must be struck, once the British connection is left behind in 1776, by the insularity (if a British reader may be allowed to use the term) of this impressive corpus of scholarship. The eye that probes so sharply into every corner, every aspect of the national experience is seldom turned outside as well. Of the historians here not explicitly concerned with extra-American relationships, very few seem interested in relating the American experience either in its uniqueness or its similarities to the experiences of other societies of the same or earlier periods. Professor Garraty very seldom asks his scholars how their findings about America compare with their own or other historians' findings about Australia, Russia, Latin America, or even Canada. Nor would the New Left, had it been called upon to testify, have altered the balance significantly in this particular.

It is interesting to speculate how this set of conversations would compare with a similar set conducted with British historians. I suspect that for a variety of reasons there would have been less cohesion, less of the feeling of a closely knit guild, about the British volume. In part of course this would reflect the greater duration of the period, the greater difficulty of communication between the historian of Hengist and Horsa and the analyst of Britain between the wars. But I suspect that another factor would be at work. I doubt whether, despite the territorial compactness of much of his subject, the British historian feels himself, to quite the same degree, obliged to assume the role of national interpreter, of elucidator of the national experience, as does his American

counterpart. He sees himself—perhaps to his own detriment—much more the expositor of the given than the analyst of the made. By contrast Professor Garraty's subjects, for all their diversity, behave and feel, in an almost Gallic sense, as if they were the lay *clerics* of the nation. If that is so, their alertness, noncomplacency, and critical integrity are a guarantee that from this quarter of the American establishment, at least, there will be no risk of *trahison*.

New College, Oxford

H. G. NICHOLAS

THE ALLEGHENY FRONTIER: WEST VIRGINIA BEGINNINGS, 1730-1830.

By *Otis K. Rice*. (Lexington: University Press of Kentucky. 1970. Pp. xiii, 438. \$10.50.)

PROFESSIONAL historians who write about frontier or regional history often obscure the subject because of their concern with the frontier and sectional theories of Frederick Jackson Turner. Most nonprofessionals who deal with the same subjects obscure them by providing an overwhelming array of disjointed facts unrelated to each other or to broader historical considerations.

In *The Allegheny Frontier*, Otis K. Rice has avoided both these pitfalls and has made a valuable contribution to historiography as well as to the history of a frontier. Without becoming enmeshed in the controversy over the "frontier thesis," he describes the forces and motives that propelled settlers across the Alleghenies, the "barbarous circumstances" that they met there, and their efforts to adjust to them. Rice's basic conclusion is that while the settlers were able to devise means by which they could survive in the less than hospitable mountain region, the very means they did devise added to the geographic isolation of the area, created a case of arrested development that left the entire area far behind the rest of the state and nation. His chapters on the evolution of "ways and folkways," especially those on medicine, education, and religion—very interesting in themselves—substantiate his analysis. Rice makes clear, however, that the unresponsiveness of the government of Virginia to the problems of its western counties and the general lack of interest by the national government contributed, as much as local circumstances, to the region's arrested development.

While Rice's analysis is in no way teleological, he shows quite clearly that many issues were more important than slavery in prompting the residents of the mountain counties to take advantage of the secession crisis in order to separate from Virginia and establish a state of their own. Similarly, he shows that many of Appalachia's contemporary problems have their roots in the region's first century of development.

Rice does not attempt a detailed analysis of the region's economy, and this may well be the major fault of the book. Yet, it may well be argued that a full-scale discussion of an economy that did not progress beyond subsistence agriculture is hardly necessary. In short, this is an excellent work and model for other historians studying regional or frontier history.

Western Carolina University

VICTOR SAPIO

TRADE AND PRIVATEERING IN SPANISH FLORIDA, 1732-1763. By *Joyce Elizabeth Harman*. (St. Augustine, Fla.: St. Augustine Historical Society. 1969. Pp. vii, 99. \$3.00.)

PRIVATEERS IN CHARLESTON, 1793-1796: AN ACCOUNT OF A FRENCH PALATINATE IN SOUTH CAROLINA. By *Melvin H. Jackson*. [Smithsonian Studies in History and Technology, Number 1.] (Washington, D. C.: Smithsonian Institution Press. 1969. Pp. viii, 160. \$3.50.)

THESE two interesting studies have one thing in common: they both deal with the history of privateering in the eighteenth century along the southeastern Atlantic coastline of the continent of North America. In each work, also, abundant information is supplied concerning privateering vessels and the nature and extent of the operations involved. In other respects, however, the two authors are concerned with quite widely separate fields of history and with varying purposes and objectives.

Joyce Elizabeth Harman's story of trade and privateering in Spanish Florida during the second third of the eighteenth century is an undertaking commenced at the suggestion of her adviser, Professor John J. TePaske, at Ohio State University. It demonstrates the undoubted value to a struggling and neglected colony of an extensive trade conducted largely in privateers from the ports of English colonies northward along the Atlantic seaboard. For, without this generally illegal commerce, and in view of the all too frequent failure of the annual subsidy (the *situado*) to arrive from the Indies, the inhabitants of St. Augustine and its environs might not have been able, as is pointed out, to maintain Spain's hold on this strategically important outpost until 1763. Despite gaps in the records, particularly for the years 1739 to 1752, the author accounts for approximately 116 cargo vessels, chiefly English in origin, arriving at or leaving from St. Augustine in the period covered by her study.

Melvin Jackson, curator of the Marine Transportation National Museum of History and Technology, has used his wide knowledge of privateering and marine history to pinpoint conditions in Charleston in the years 1793 to 1796, when the young American nation was under heavy pressure from the French to violate rules of neutrality and actively to aid in fitting and sending out vessels from that port, among others, to engage in open warfare against the enemies of France. Jackson's careful study reveals especially the aggressive role of the French consul, Mangourit, as well as that of Fonspertuis, his more restrained successor, both of whom were helped considerably by strong local pro-French sympathies.

Silver Spring, Maryland

VERNE E. CHATELAIN

THE PAPERS OF BENJAMIN FRANKLIN. Volume XIII, JANUARY 1 THROUGH DECEMBER 31, 1766; Volume XIV, JANUARY 1 THROUGH DECEMBER 31, 1767. *Leonard W. Labaree*, Editor. *Helen C. Boatfield* and *James H. Hutson*, Assistant Editors. (New Haven: Yale University Press. 1969; 1970. Pp. xxvii, 580; xxviii, 382. \$17.50 each.)

WITH these two books, Leonard W. Labaree and his two assistant editors conclude their monumental contribution to the publication of Benjamin Franklin's papers. Exhibiting consistently the highest form of editorial scholarship, the first fourteen volumes are a splendid accomplishment. They furnish Professor William Willcox, the new editor, with both his challenge and his inspiration.

Volumes XIII and XIV span the years 1766-67. The increasingly complex and dan-

gerous issue of parliamentary taxation provides a kind of leitmotiv. In this context, Franklin's actions in the Stamp Act crisis are especially important. During his examination before the House of Commons in February 1766, for example, he laid down the celebrated distinction between "internal" and "external" taxes. Pushed to its logical conclusion, the argument was an implicit questioning of the power of Parliament. Only a few months later it became explicit. With the repeal of this odious act and the passage of the Declaratory Act accomplished facts, Franklin addressed himself (in marginal comments) to the protest of the opposition peers. The American people had never given the British Parliament the right to tax them, he wrote. Their obedience was to the Crown alone. Since the matter was in dispute, it was now necessary to devise a new colonial constitution.

For the time being, these were only private thoughts. As late as April 1767, writing as "Benevolus" in the *London Chronicle*, he avoided a flat condemnation of parliamentary authority, resting a major portion of his case on the distinction between external and internal taxes. The colonies had, he said, rather inaccurately, always obeyed "all acts of the British Legislature, expressly extending to the Colonies, . . . the right of Parliament to make them being never yet contested, acts to raise money upon the colonies by internal Taxes only and alone excepted" (XIV, 114).

When Parliament appeared to take Franklin at his word and passed Charles Townshend's "external" duties, the American's response was surprisingly mild. He was deeply engaged in negotiations with the Board of Trade about grants in the Illinois country, of course; but other more important reasons doubtless distracted him. Chatham, America's friend and titular head of the ministry, had collapsed physically and mentally. Parliament's efforts to coerce the New York Assembly into obedience to the Mutiny Act was especially alarming. Finally, there was the Grenvillite threat to raise a revenue from America by permitting the emission of paper money—a favorite colonial object—subject to the payment of interest. The Townshend taxes were, for Franklin, but one indication of the grave dangers ahead.

Aside from questions of high policy, there is an abundance of personal information about Franklin the man. Should one wish to know how Parisian ladies rouged their faces, the secret is laid out in an account written during a visit to France in the late summer of 1767. Present at the *Grand Couvert*, he was presented to Louis XV, whose favorable impression still did not impugn Franklin's love for George III, who of all monarchs was "the very best in the World and the most amiable" (XIV, 252). Here spoke the still loyal subject.

Southern Methodist University

CHARLES R. RITCHESON

THE FAIR PLAY SETTLERS OF THE WEST BRANCH VALLEY, 1769-1784:
A STUDY OF FRONTIER ETHNOGRAPHY. By *George D. Wolf*. (Harris-
burg: Commonwealth of Pennsylvania, Pennsylvania Historical and Museum Com-
mission. 1969. Pp. x, 122. Cloth \$3.50, paper \$2.50.)

MR. Wolf describes himself as "practically a life-long resident of the West Branch Valley," and this book is a labor of love. Ostensibly it is an exercise in descriptive anthropology tracing the development of a community established in a twenty-square-mile tract on the Pennsylvania frontier. Although the conception is an interesting one, it is developed unsatisfactorily because Mr. Wolf lacks sufficient literary skill, sufficient methodological sophistication, and, most crucially, sufficient data. Because of the paucity of evidence no professional historian would have attempted this topic. For ex-

ample, the "fair play system" itself, from which the settlement takes its name, was administered by an elected board of three "fair play men" that settled land disputes and possibly other controversies as well in the years before the territory came under Pennsylvania jurisdiction. There is no evidence relating to suffrage qualifications or qualifications for holding the office of "fair play man," no record of any election being held, no code of laws and only a few identifiable legal principles, no evidence of any specific meeting of "fair play men," and except for 1775 and 1776 even the names of the "fair play men" cannot be determined. Wolf must base his description of the settlement's political life on the records of four property cases later appealed to Pennsylvania county courts and a few hearsay anecdotes. Wolf's attempts to manipulate his inadequate data are awkward. He consistently makes the logical error of arguing from absence of evidence and bases elaborate analyses on tables he himself recognizes are "intuitively rather than statistically sound." The book fails in its larger purposes of providing "support for Turner's thesis" and establishing "a further technique for the frontier historian," but the new data Wolf has collected may be of interest to students of local Pennsylvania history.

George Washington University

LINDA GRANT DE PAUW

FACTORY UNDER THE ELMS: A HISTORY OF HARRISVILLE, NEW HAMPSHIRE, 1774-1969. By *John Borden Armstrong*. (Cambridge, Mass.: M.I.T. Press for the Merrimack Valley Textile Museum. 1969. Pp. xx, 320. \$12.50.)

In his study of the small textile town of Harrisville, New Hampshire, John Borden Armstrong has drawn on newspapers, interviews, and census reports, and has depended upon town and company records. One of Harrisville's two mills left no adequate records, and few collections of letters were available. Despite these handicaps, the author has provided a minute description of the village.

Armstrong has done his research well, uncovering useful information about wages, town budgets, property valuations, and vital statistics. He adds careful vignettes to set the scene. His book covers a narrow subject broadly. Unfortunately, Armstrong fails to generalize effectively from this data. Typical is the chapter on society, 1870-1900, in which he discusses houses, roads, doctors, tramps, immigrants, and alcohol, among other subjects. Fascinating hints are there: money spent on liquor increased four times; the school year expanded from eighteen to thirty-two weeks; tramps became a problem during the depression of 1894. Historians are anxious to discover what happened to values and community spirit as industrialization progressed, but Armstrong offers few answers.

Sometimes his sentiments show. Immigrants were not always tidy, spinners were noted for "arrogance," but the leading mill owner was "kindly and considerate." After pointing out that wages were low, that fines were frequent, and that workers struck three times in the period 1887-90, he concludes that employees in Harrisville were contented. He quotes a letter from a "spy" to one owner reporting that spinners were cheating the company. Armstrong argues that the letter was "unsolicited," and that it shows that the spinners' "bad reputation" was deserved. The evidence, however, makes Harrisville sound like a company town.

In his epilogue the author states that the "significance of this small community lies in its history of adapting to changing circumstances without destroying the past." Armstrong has documented the changes, but has not reflected sufficiently on their significance.

Phillips Exeter Academy

DONALD B. COLE

PATRICK HENRY: PATRIOT AND STATESMAN. By *Norine Dickson Campbell*. (New York: Devin-Adair Company, 1969. Pp. xvii, 437. \$10.00.)

BIOGRAPHERS of Patrick Henry, from William Wirt to the present, have all shared certain frustrations. The oral traditions have been numerous, but the facts have been relatively few. As Wirt complained in 1815, "not one of his speeches lives in print. . . ." Nor was there much in the way of revealing information on Henry as a military man, governor, or working politician. "In short," Henry was "as hopeless a subject as man could well desire." Indeed, it may well be true, as the late Douglass Adair once suggested, that Henry is an impossible subject for a narrative biography. Instead, Adair urged scholars to raise analytical questions about the so-called "Forest-born Demosthenes" that seek to explain his activities within the framework of eighteenth-century Virginia society and politics.

Yet it is the character of the man and the formative influences of his youth that continue to attract—and baffle—historians. Robert D. Meade's recent two-volume life of Henry, based on exhaustive research, fills out the public record, but it scarcely reveals the inner man. The year 1969 witnessed the appearance of two additional biographies of Henry: George F. Willison's *Patrick Henry and His World* and Norine Dickson Campbell's *Patrick Henry: Patriot and Statesman*. Unfortunately, neither book escapes the romantic influence of Wirt, despite the authors' claims to the contrary.

Mrs. Campbell, a resident of Henry's own Hanover County, Virginia, has long maintained an interest in her subject. She played a large part in securing and restoring the ancestral Henry home, Scotchtown, for the Association for the Preservation of Virginia Antiquities in 1958. Perhaps for these reasons she writes with a good deal of warmth and charm about Henry family matters and local affairs. Persons with an antiquarian interest will no doubt find much to enjoy in this handsomely illustrated biography. Scholars, unfortunately, will discover it to be just another Henry biography, heavily footnoted but offering little that is new and repeating too much that is old.

University of North Carolina, Chapel Hill

DON HIGGINBOTHAM

NAVAL DOCUMENTS OF THE AMERICAN REVOLUTION. Volume IV, AMERICAN THEATRE: FEB. 19, 1776–APR. 17, 1776; EUROPEAN THEATRE: FEB. 1, 1776–MAY 25, 1776; AMERICAN THEATRE: APR. 18, 1776–MAY 8, 1776. *William Bell Clark*, Editor. With a foreword by *John H. Chafee* and an introduction by *Ernest McNeill Eller*. (Washington, D. C.: [Navy Department.] 1969. Pp. xxxi, 1580. \$14.25.)

I HAVE been critical of the *Naval Documents of the American Revolution* project in the past on various grounds: the use of inaccurate transcripts when copies of original documents were available; the failure to make a systematic search of all available manuscript collections; the breadth of the project that encompasses naval warfare on inland lakes when coverage of activities on the high seas might have been an ambitious enough scope for the series; the Whig bias betrayed by the editorial staff in its selections and commentaries; and the inadequacy of the index as a research tool to reveal important categories contained in these volumes. Although the Naval History Division has corrected many of these shortcomings, the project still does not measure up to the high editing standards established by the magnificent Adams Papers, Jefferson Papers, Hamilton Papers, Franklin Papers, and Madison Papers, among others.

One reason for the continued criticism of this project is its tendency to perpetuate old errors. Transcripts are still being reproduced where the original documents, no

doubt, are available. Such is the case of the Sparks Transcripts of Lord Grantham's correspondence at Harvard (p. 904); the letter from Force's *American Archives* (p. 1184); and the letter by Lord George Germain (pp. 879-81) reprinted from Baxter's documentary *History of Maine*. A Whig bias still is evident in the work: the statement that the Americans were a "determined people" in the spring of 1776 is hardly accurate; and the claim that the "blood clause" providing payment to German princes for any mercenaries killed, wounded, or captured in America was "shameful," is uncalled for. The index, though considerably improved, does not list, for example, Baron Albert de Dorlodot of Belgium; nor are we told in the text where the Baron's scholarly research on the validity of the Beaumarchais letter in question may be found (p. 929).

A second reason for faulting the project is on the physical appearance of these volumes. Some copies of Volume IV, by Naval History Division's own admission, came off the press with "paper, printing, and binding irregularities." Must future historians cope with defective volumes in a reference work of this sort? The volumes themselves, moreover, are far too bulky and will not stand up to continued use. Finally, some of the illustrations—such as Appendix C and the manuscript map of Newport, Rhode Island, by Ezra Stiles—are illegible.

Volume IV carries the announcement of the death of William Bell Clark, who helped to get this editorial project underway a decade ago. Author of a number of books and biographies dealing with naval developments in the Revolutionary War, Clark collected thousands of pages of transcripts on this subject and period that served as the basis for research in beginning this series. No better tribute could be paid to Bill Clark by the Naval History Division than to improve the scholarly standards of the project that bears his name.

Clark University

GEORGE ATHAN BILLIAS

THE CAMPAIGN THAT WON AMERICA: THE STORY OF YORKTOWN. By *Burke Davis*. (New York: Dial Press. 1970. Pp. 319. \$8.95.)

CORNWALLIS: THE AMERICAN ADVENTURE. By *Franklin* and *Mary Wickwire*. (Boston: Houghton Mifflin Company. 1970. Pp. xvi, 486. \$10.00.)

WITH the approach of the bicentennial of the American Revolution, an abundance of books on the subject may be expected, and this is as it should be. It is hoped that they will not move the celebration as far out of proper perspective as some volumes did for the Civil War. The two books named above fit nicely into the advance guard of the procession.

The work of Burke Davis is a vivid, commendable account of the battle that virtually ended the Revolution in America. Although the peace was not formally forthcoming until more than a year later, the surrender of Lord Cornwallis at Yorktown on October 19, 1781, was a salient signal for the termination of hostilities and the eventual downfall of the North cabinet. At long last, George III was reluctantly convinced that the colonies would not accept his dicta.

Burke Davis, a popular author, is in an excellent position to write about Yorktown and Cornwallis, being an official of Colonial Williamsburg. Using colorful original sources, especially French ones, he has presented a narrative that not only adds to our knowledge of the encounter, but with journalistic flourishes, he has produced an entertaining story as well.

The historical facts of Yorktown are well known, so Davis turns his attention to the

humor, the pathos, and the strong adventure that surrounded the sequence of events. This he deftly achieves by concentrating frequently on the personalities involved. George Washington is the central figure, shown here in revealing, informal moments of joy and despair, playing ball with his officers and dancing gleefully when he learns of the approach of the French fleet. General Rochambeau is portrayed as a gracious gentleman in his relations with the American leaders but as a stern if respected "Papa" to his own men. Lafayette is overplayed as usual, but his youthful characteristics and his appeal to Washington are the subject of interesting appraisal. Sir Henry Clinton comes off as a bumbling, indecisive general in both books, although his biographer has credited him with winning what is termed the most important British victory, that of Charleston.

The Wickwire volume is probably the most complete account of this able and often effective British general, although the digressions from the main subject sometimes tend to be unnecessarily detailed. The authors, man and wife on the faculty of the University of Massachusetts, strive hard to vindicate Cornwallis but generally fail to do so. They have done deep research in English sources and have compiled this in eighty pages of notes at the end of the book, almost one-sixth of the entire volume. The notes are at times unbalanced, contain some errors, and quote Kenneth Roberts, the novelist, as a possible authority. Some of the secondary sources used are overworked, while other good ones are omitted. Cornwallis is excellently described as a personality, and his Southern campaigns are set forth in close and poignant detail.

These two volumes, while dwelling mainly on the personalities of the Southern campaigns, add to the human interest of the Revolution. The role of the artillery is properly emphasized in the Davis volume, not in the other. The excellent research and the resulting narratives make a valuable contribution to the general knowledge of the virtual ending of what was in some ways our most important war.

New York University

NORTH CALLAHAN

THE JUSTICES OF THE UNITED STATES SUPREME COURT 1789-1969: THEIR LIVES AND MAJOR OPINIONS. In four volumes. *Leon Friedman* and *Fred L. Israel*, Editors. With an introduction by *Louis H. Pollak*. (New York: Chelsea House Publishers in association with R. R. Bowker Company. 1969. Pp. xxiv, 814; vii, 817-1630; vii, 1633-2443; vii, 2447-3373. \$110.00.)

FACULTIES and campuses that no longer are taking governance and magistracy for granted at last can sympathize with the Supreme Court. The justices always have been—and under our government, must be—magistrates and educators in one. Chief Justice Waite's classic discovery and recognition of this—"The difficulty with me is I cannot give the reasons as I wish I could"—impresses us perhaps more than it did his addressee, the articulate and experienced Justice Field. This massive, uneven work shows on every page just how awesome and varied the Court's burdens and duties are, how hard it is for all now jointly reinvolved to "give reasons" and to cover everything well.

Reference works generally are hybrids that mature slowly. This one is gargantuan at birth: 3,400 pages; nearly two million words; ninety-eight lives, averaging just over 7,000 words and including Judge Haynsworth, are printed in the order of the appointments. This ingenious turn works admirably, as can be seen best in the strong dovetailing coverage of the formative period, especially in the essays of Irving Dilliard (Jay, Samuel Chase), Michael Kraus (Paterson, Ellsworth), Gerald Dunne (Livings-

ton, Story, Thompson), and others mentioned later. In all there are thirty-eight contributors, with academics predominant. Front-rank, recent, and current justices generally have been assigned to specialists or to previous biographers, and nearly all are well covered. Less distinguished justices, and even some outstanding ones of the Taney, Waite, and Fuller courts especially, have been assigned *en bloc*, as many as eleven or eight to a contributor, with unhappy results. Finally, one or more of each justice's opinions—189 in all—are printed in full. The four hundred volumes of the Supreme Court Reports thus are reduced physically to two; most of our basic constitutional law is here, although opinions in some cases are scattered or wanting and in others are poorly introduced.

Reference works of course are also publishing's steeplechases—"only finishers win." Hopefully but not unmistakably the Xerox-Bowker Sweepstakes, this set shows how mischievous a sudden and overneat periodizing of history—1789-1969—can be. Work had begun well beforehand, but the announcement of Chief Justice Warren's pending retirement, with ensuing events, obviously created compulsions and deadlines. Effects of these are manifest most conspicuously in the *en bloc* Taney-Waite and minor-justices sections. In their present form twenty-two of these essays serve well neither the subject-members of the Court, the authors, nor the enterprise. Another fifteen are inadequate or disappointing, occasionally in contrast to the authors' other essays. Production editors charged with approving the final press order thus faced impossible compromises. The pity is that several able political historians and period specialists, caught overextended with fractious judicial mounts (ridden at times as if still in Congress or in the cabinet) have come a cropper. Lax writing, loose editing or none, flagrant misprints and misstatements, garbled paragraphs, weak case discussion, "Selected Bibliographies" that plainly are neither, all betray the haste and the spurs. A finish as old as publishing, yet dismaying and unworthy still.

The quality curve thus resembles a flattened, palsied "w" or "double u": a strong, fairly even start (Jay through Thompson); the sagging Taney court, relieved by another plateau (Davis through Hunt, Stanley Kutler's series in particular); then a shorter, more uneven sag, one ending in 1900, and relieved before that by Arnold Paul's strong and well-integrated group (Blatchford, Lamar I, Brewer, Shiras), and by Irving Schiffman's excellent Howell Jackson—a justice who served barely two years, yet whose life and dramatic dissent in the second *Income Tax Case* are nicely covered (in contrast to the hurried, disappointing Fuller). Unevenness thus is the hallmark; redoubled assignments and pressures often have quartered quality.

Even so, intuitive editors have proved their point: collective biography may be a poor medium for a history of the Supreme Court, but it is an excellent one to clarify and project the role of the Supreme Court in United States history, an even more difficult and vital task. Here, accordingly, on a much more elaborate scale than in Charles Warren's work, are covered the origins, the uniqueness, the mechanics, and the 180-year practice of federal judicial review and supremacy. From these extensive records and reassessments the institutional function and picture emerge. The *Lives and Opinions* of course also do double duty as reference sources. Steeplechase or not, disappointments notwithstanding, we thus possess an important new resource and tool. Allowing for the hazards, 60 per cent "strong finishers," 40 per cent "other," may be a record first run.

The periodists' essays generally are the most dated and uneven, individually and as a group, with the *en bloc* series weakest and most unfocused judicially. Three essays by Richard Kirkendall (Vinson, Clark, Minton), all strongly crafted and trended,

even researched in the Truman Papers, are exceptions. David Burner's Sutherland, Clarke, and McReynolds are better than his hurried Van Devanter, Butler, Sanford, and Roberts. Diplomacy and politics are interestingly covered by James Watts, Jr. (Day and Moody), but the jurisprudence and vocabulary are naive, and the essays on McKenna and Chief Justice White particularly are loose, ill-proportioned, and wholly without copyediting (for example, "sugar beet plantations" are referred to for sugar cane in White's Louisiana). Yet, Leonard Dinnerstein's outstanding Lamar II and Leon Friedman's strong essays on Bradley and Chief Justice Chase, with Fred Israel's suggestive coverage of Thomas Todd and Pitney, show what might have been—what still can, what must be.

Mood in the more pedestrian essays occasionally turns "mod" and the discourse atavistic. Thus, introducing Justice Peckham, Richard Skolnic revives the Conspiracy Theory and impersonalizes it: not Roscoe Conkling now, but a ruthless, frightened "corporate America" and a "determined band of corporate lawyers" prepsed everything, "and the Supreme Court emerged as the principal agency committed to ratifying" just that. (For thirty years' unrequited labor in this vineyard, see Graham, *Everyman's Constitution* [1968].)

"Elites" and "Establishments" also get pejorative needles, even some of the Jacksonian persuasion, and often craftsmanship may suffer too. Brandeis' briefs are cited and hailed for half-converting the Court and public, but with the force of example or inspiration often lost. Forty years on, Parrington and Charles Warren are less than the last word, yet they still often have it in these sections and in preference to the classic works of Corwin, Haines, Hamilton, Powell, Frankfurter and Landis, Hurst, Dodd, Henderson, Commons, Hale, Hartz, Goodrich, White, and others, whose writings in constitutional, legal, economic, and administrative history have reshaped scholarship generally, but seldom have reshaped it here. Among matters in need of coverage or of much stronger treatment in these sections are the expanding business of the Supreme Court; the rise of the corporation; the shifts of charter policy and diversity jurisdiction; the parallel, interacting lines of economic and humanitarian due process—pre- and post-Civil War—with innovating usages often in Congress and in the state courts; and the role of the Circuits and of the "Circuit law." It is unfortunate, for example, that overcommitment should have prevented Louis Filler from capitalizing on regional and subject interests and from providing a more adequate coverage of Harland I and Gray, and of the three Ohio justices (Waite, Woods, and Matthews), on all of whom important work has recently been done, some cited here but unassimilated, some not cited at all.

In the last two volumes especially, delights and achievements abound. Among the most memorable are Paul Freund's Justice Holmes in seven incomparable pages and Philip Kurland's Justice Jackson, which is brilliantly edited from oral history tapes; a gallery of biographic reprises highlighted by Alpheus Mason's elegant triptych—Taft-Stone-Brandeis—and by Anthony Lewis' Chief Justice Warren, these entries brilliantly supported by Samuel Hendel's Hughes, Andrew Kaufman's Cardozo, Herman Pritchett's Reed, and by the late Robert McCloskey's Wilson and Field. Four former law secretaries contribute outstanding studies of Justices Frankfurter (Albert Sacks), Harlan II (Norman Dorsen), Black (John Frank), and Stewart (Jerold Israel). Frank also writes expertly, *con amore*, of Justices Douglas and Murphy. Together with Stephen J. Friedman's studies of Justices Brennan and Goldberg and Walter Murphy's Byrnes, which nicely assimilate the various decisional analyses and trends of the Stone-Vinson-Warren period, these essays all develop the collegiate character, the diversity and com-

plexity of the justices' work, and do this by establishing individual roles, specialties, approaches, and craftsmanship. Even the juxtapositions often reinforce harmonically. Few reference sets and no works of collective biography have ended more spectacularly. The three final essays, all excellent, are by reporters of the *Washington Post* and the *New York Times*: Justice Fortas by Fred Graham, Justice Thurgood Marshall and Chief Justice Burger by John P. MacKenzie. The irony and nuances of the unprecedented Burger installation ceremony, consummately covered, even provide a coda.

Citation practices, references, and indexing generally are lax, sometimes self-defeating. (The heterogeneous and unpredictable market perhaps is partially to blame.) The Supreme Court Reports are habitually cited. On original or crucial points the better essays usually give further support, but not always; a number do so rarely, if at all. Original research contributions thus were unfavored, or worse, were diluted and partially lost. Unfortunately this happens with Herbert Johnson's often fresh views of John Marshall and Thomas Johnson, Blaustein and Mersky's strong Washington, and Gerald Dunne's excellent Livingston.

Skillful planning and editorial use of the direct reference *q.v.* would have improved usefulness and coverage throughout. Indexing, though extensive, is also unimaginative, being listed almost entirely by justices and cases with numerous page entries made for merest mentions, yet without effort to record, still less to collate, fascinating and significant data on such recurrent matters as duels and duelists (yes, two!), political aspirations and candidacies, judicial craftsmanship, extrajudicial services, and judicial decorum. Incidentally, two things most strikingly shown here are the regularity with which members of the Court, from 1789 to date, have been drafted for extrajudicial assignments, few if any of which comport well with our professed adherence to the separation of powers; and second, the stricter, tightening public and personal standards of judicial disinterestedness and decorum. In 1828, Smith Thompson, as a sitting justice, ran for governor of New York against Van Buren and polled 106,000 votes, egged on by Marshall and Story!

In the appendicized charts and tables repetition proves no substitute for precision. Precise dates of birth, nomination, death, termination, and length of service are not given; often these can be found with difficulty, occasionally not at all, in the texts. Uniform inclusion of this data on the title pages of essays surely is the preferred solution in an authoritative work.

Xeroxed Bowker, in short, often is something less than vintage reference Bowker. Yet, we are thankful for this set and look forward to its improvement. Unfinished it is and will always be, for the work of the Court, and of academia, is inherently so. John R. Commons put it best, and gallantly: "The Supreme Court of the United States . . . occupies the unique position of the first *authoritative* faculty [of law, government, and economics] in the world's history." Educators *and* magistrates!

Los Angeles, California

HOWARD JAY GRAHAM

EARLY JAZZ: ITS ROOTS AND MUSICAL DEVELOPMENT. By *Gunther Schuller*. [The History of Jazz, Volume I.] (New York: Oxford University Press. 1968. Pp. xii, 401. \$10.50.)

GUNTHER Schuller's history of early jazz is the most scholarly and perceptive work on the subject to date. One can imagine the collective tossing of caps into the air that the above statement will produce within the profession. For older historians the term "jazz" may evoke hazy memories of doing a snappy foxtrot to "Harry Horlick and His

A&P Gypsies"; the young are vaguely aware that jazz was some kind of precursor to the more relevant joys of rock. Our young black scholars are more knowledgeable, of course. They know that jazz was invented by Charlie Parker, solely to express black America's profound contempt for everything white. They know this because LeRoi Jones has told them so. All might benefit from this beautifully written combination of precise musical analysis and sensitive appreciation.

Schuller argues convincingly that every musical element in early jazz is essentially African in derivation. His analysis of African music is quite technical, and many readers will have to apply the great lesson learned from recent quantitative studies in history: skip the devilish rigmarole of figures and hope for a lucid explanation of their meaning. In America minstrel music, brass bands, ragtime, and the "blues" antedated and influenced the music that became jazz. Schuller speculates about such influences and proceeds to a brilliant description of "classic" New Orleans jazz—polyphonic, highly disciplined, predictable, and collective rather than individualistic. King Oliver's Creole Jazz Band is examined as the quintessential classic New Orleans band.

It was the excitement of the true solo and improvisation (as opposed to mere "embellishment") that took jazz beyond the classic New Orleans style, and it was fitting that the cake of custom was broken by King Oliver's protegee—Louis Armstrong. A chapter titled "The First Great Soloist" analyzes Armstrong's musical growth from second cornetist with Oliver to the famous Chicago "Hot Five" and "Hot Seven" recordings. The stunning Armstrong introduction on the 1928 "West End Blues" symbolizes for Schuller the emergence of jazz as authentic art; a performance that established the stylistic direction of jazz for the next decade. The only significant flaw in an otherwise masterly discussion is the author's opinion of the post-1929 Armstrong.

Jelly Roll Morton always insisted that he invented jazz, among other things. Morton's grandiose assertions are rather like the myths of classical antiquity; one doesn't quite dare not believe them. Schuller, in a provocative chapter titled "The First Great Composer," characterizes Morton as "the first theorist, the first intellectual, that jazz produced." His thesis that Morton's Creole prejudice against "black Uptown Negroes" was at once his strength and his weakness—responsible for his greatest recorded achievements and his arrested musical state—is fascinating.

Other chapters treat "Virtuoso Performers Of The Twenties," "The Big Bands," and "The Ellington Style." Highlights include a beautiful little piece on Bix Beiderbecke, a valuable survey of the big bands of the Southwest, and fresh material on the early ducal style.

Complete understanding of Schuller's work dictates not only a knowledge of music, but actual possession of the records he analyzes. Few readers will be in that position, but one of the virtues of this book is that the layman can share with the expert many of the benefits of Gunther Schuller's profound understanding of early jazz.

University of Northern Colorado

GEORGE A. BOECK

CORRESPONDENCE OF JAMES K. POLK. Volume I, 1817–1832. *Herbert Weaver*, Editor. *Paul H. Bergeron*, Associate Editor. (Nashville: Vanderbilt University Press, 1969. Pp. xxxviii, 619. \$15.00.)

JAMES K. Polk in 1844 was our first dark horse candidate for president, and he remained an obscure figure until our own times, when he was rediscovered as a strong and perhaps even great president. For a number of years Professors Herbert Weaver and Paul H. Bergeron of Vanderbilt University have been at work collecting Polk's

correspondence, and we have now the first volume of that project, handsomely published by their university's press. Spanning the years 1817 through 1832, the book contains 664 letters. Ninety-six of these were written by Polk himself and are reproduced in full, while of the rest 138 are summarized. Most of Polk's own letters are in the Library of Congress in his own and Andrew Jackson's papers, but the editors have searched the country for other items from such diverse repositories as university libraries; state historical societies, libraries, and archives; and the National Archives. To our great benefit, and for the first time, they have brought together all of Polk's private and public correspondence.

A reading of this volume suggests why Polk was for so long an unknown figure. During the period there is only one letter from Polk to his wife, only one letter from his mother, and meager—though sometimes illuminating—family correspondence. It would be hazardous to reconstruct an internal life from these without the most sensitive and probing psychoanalytical tools. Yet the challenge is clear: a mother Polk always referred to as “the old lady,” an alcoholic brother, only half of six sons marrying, Polk's wracking illness, a gargantuan and insatiable (in more ways than in land) grandfather, an ambitious father, a childless James K. Polk who raised younger brothers when his own father died. The materials on Polk's early political career are fuller, and these letters tell us much about the still new state of Tennessee, which was being exploited through ruthless land grabbing in which the Polks participated. They also reveal the slow shift in Polk's political philosophy from his holding on to Jeffersonianism past its time to his being buffeted by incipient Jacksonianism.

We have in these writings some of the building blocks of Polk's personality and political life, but the editors eschew any comments, any interpretation, any argument or agreement with what recent historians have done with these documents. Professor Sellers' intuition that Polk the man was shaped as Polk the boy in the air of Mecklenburg, North Carolina, finds little support in this volume. On the contrary, one could turn this around and say that a Presbyterian matriarch and the frailties of Polk's constitution formed an insecure, unoriginal boy who even with some education and experience did not become less pedestrian but only developed a doggedness of character and enough insight to warn him to hold on to what he had. Nor is there much support here for the Dorfman-Hammond-Hofstadter thesis that the Jacksonians spoke for the rising middle class, if Polk is at all representative. In modern idiom, Polk's never fully formed ego clutched the reliable, seldom if ever let down its guard, and was adventurous not at all. These letters seem to indicate that Polk's inner necessities, perhaps more than what happened around him, made him a sturdy Jeffersonian, a late Jacksonian, and, we may find out later, a reluctant imperialist. There is strong evidence that Marvin Meyers' theme of restorationism elegantly suits Polk's career.

None of these issues, however, are even considered by the editors of this volume. Astonishingly, the entire preface to the series and volume runs only seven pages, almost equally divided among a biography of Polk, an explanation of letters included, and the editorial method, a bibliography, and acknowledgments.

It can be argued, of course, that the real task of the editors was the accurate presentation of the letters with adequate annotation. Yet as for accuracy, one hardly knows where to begin because the editors have not explained in any detail their editorial method. In checking over the few photographs of letters given as illustrations with the printed text, I suspect too many liberties have been taken to modernize the correspondence. Punctuation and capitalization were uneven and bizarre then and may have been tampered with unduly. We must assume that the collation of the manuscripts has

mostly been well done. But why read on page 417 "He had law in my office. . . ." when the illustration of the letter clearly shows the writer said "he read law in my office. . . ." Or why tamper on page 164 with "County" to make it "Count[r]y"? I notice other differences, but I doubt they change too much the sense of the letters.

The criteria by which letters are included in this volume are faulty. Too much incoming mail has been presented in full. Many more letters ought to have been summarized in order to give more room for Polk's own writings. If the editors had done this, the emerging national leader would not be left dangling at a critical juncture of his political life. To be blunt, ninety-six letters of James K. Polk do not warrant a volume in themselves.

If the greatest disappointment of this volume is the paucity of Polk letters, a second unhappiness derives from inadequate annotation. The editors have been most industrious about locating and identifying persons of marginal as well as of great importance. But either they assume that we know a great deal about Tennessee politics and national happenings, or else they construe their job narrowly. There are literally dozens of events in the Polk correspondence that need explication and cross references. Throughout the early years we need some annotation to guide us through the thickets of Tennessee politics; family affairs are too tangled because of scanty notes; and on national politics and sometimes international affairs we could use some aid. For example, full and exacting notes are needed on local factional politics, federal land policies and speculators, infamous handbills and various resolutions of lobbies, congressional president-making, party building, speeches in Congress, the tariff and United States Bank battles, and the British orders in council. Some short references there are, but they are perfunctory.

This volume raises some serious questions as to where the profession is going as more and more editorial projects are being undertaken. Can we not formulate some general rules for the editing of the papers of secondary American figures? Ought not our new projects to focus more on the subject himself, with incoming correspondence, except for the most extraordinary letters, being summarized? Can we not demand long, substantive introductions giving a historical overview of the subject and an interpretation of the contents of the particular volume or volumes at hand? Can we not insist that editors try to pace themselves as well as have the Lincoln, Theodore Roosevelt, and Wilson editors and not take years for the production of a single volume? Can we not plead that annotation be fuller so that students can handily use the books? Should we not have a clear, detailed statement of editorial method? Ought not each volume to have its own up-to-date bibliography? Perhaps the National Historical Publications Commission, which now subsidizes with federal money so many new projects, can get a dialogue going on such questions.

Stanford, California

AIDA DI PACE DONALD

JAMES WATSON WEBB: A BIOGRAPHY. By *James L. Crouthamel*. (Middletown, Conn.: Wesleyan University Press. 1969. Pp. x, 262. \$10.00.)

JAMES Watson Webb gave up an army career and vaulted into public notice in 1827, at the age of twenty-five, by persuading Alexander Stewart, the New York merchant who was his father-in-law, to buy him the New York *Morning Courier*. In less than two years, thanks largely to a merger he engineered, Webb's *Morning Courier and New York Enquirer* won circulation leadership in the city. It was aggressive in pursuit of news, strong for Jackson, and blessed with the ablest political reporter around, James Gordon Bennett. Webb, a tall, assertive fellow with vast ambition, appeared headed

for a large place in the national history, but not for long. From 1831 on, he faded. Mismanaging his finances, he was forced to turn to the Bank of the United States for loans. His paper, hitherto as passionately anti-Bank as Jackson, suddenly favored re-chartering it. The loans became public knowledge. Bennett quit. Within a few years, the penny journals with their broad appeal to the man in the street far surpassed the *Courier* and its mercantile contemporaries in both circulation and influence. Webb, resolutely turning his back upon the major journalistic development of his era, kept his old-style blanket sheet going until it had lost most of its audience, then sold out to the New York *World* on the eve of the Civil War. He settled for a turn as ambassador to Brazil and passed into obscurity.

If there are the makings here of a worthwhile biography, they are hard to discern. Perhaps Webb's proclivity for fist fights, for speculative investments that regularly blew up, for crudely pressuring his political friends to give him high office (he thought Seward surely would favor him for ambassador to France, since he knew Louis Napoleon) might be turned to advantage in the hands of a popular writer capable of doing us a Dickensian portrait of the man. But in Professor Crouthamel's antiseptic hands, Webb comes off a solemn bore. The author is at pains to weigh the charge that Webb was bribed to switch sides on the Bank issue, and concludes that he probably was not. That seems almost beside the point. Public knowledge of the loans left the paper hopelessly compromised, and no one seems to have paid Webb much notice thereafter.

In tracing Webb's political migration from Jacksonian to Whig to Republican, Crouthamel is more comfortable than in coping with Webb as a person. Research in the *Courier* files is deftly integrated into the broader canvas, so that the book commends itself as a case study in the shifting political attitudes of the commercial class in New York during the decades leading up to the Civil War. Turning to Webb's eight years in Brazil, it may also be read as a horror story in Latin-American relations. After pressing the *Caroline* claims unduly (Webb even threatened to sever relations on his own initiative), the ambassador pocketed a major part of the settlement—and that is but one example.

Hampered by a paucity of personal papers, the author has given us a coolly detached appraisal of a public figure who, one regretfully concludes, barely merits the effort.

Columbia University

LOUIS M. STARR

GRANT AS MILITARY COMMANDER. By Sir James Marshall-Cornwall. (New York: Van Nostrand Reinhold Company. 1970. Pp. xi, 244, 20 maps. \$9.95.)

MARK Twain once said of Ulysses S. Grant, "He was the most lovable great child in the world." And while General Sir James Marshall-Cornwall—himself a distinguished British soldier of two world wars—shows a pervasive fondness for the hero of Appomattox, the book under review is at times a hard-headed and realistic summary and appreciation of Grant the soldier. It is not quite so good on delineating Grant the man and will not replace other standard works on Grant by such historians as J. F. C. Fuller, Bruce Catton, and especially A. L. Conger.

There are some serious shortcomings in this oftentimes useful interpretation. Despite a fine grasp of the practical problems confronting a commander in the field, Marshall-Cornwall does not exhibit a comprehensive or intimate mastery of the military history of the American Civil War, or of the huge number of studies dealing with it. This is apparent in the text, the few footnotes, and the pitifully inadequate bibliography. And there is a tendency to blame subordinates—especially the able George G. Meade—for

many of Grant's failures, and to gloss over controversial matters by sweeping overstatements, oversimplifications, or generalities. There are too many direct quotations and some misstatements of fact. Nor will historians, generally, agree that Grant "stood head and shoulders above . . . Lee." Seldom does the author really come to grips with Grant's complex character and traits, and consequently Grant does not emerge as a living human being.

But there are strong points in this study for which the historian should be thankful. Sir James has an excellent appreciation of terrain and geographical influences on military operations. He does not excuse or try to explain away—as do some writers—Grant's military errors, such as the Belmont setback, the surprise and failure to pursue at Shiloh, his faulty maneuvering at the Wilderness and Spotsylvania, the fiasco at Cold Harbor, his initial fumbling at Petersburg, and his unlimited backing of the limited Philip Sheridan. The author is at his best in his frequent and instructive references to parallel situations in other famous battles of history. There are numerous illustrations and adequate maps.

In sum, General Marshall-Cornwall has given us a fairly balanced analysis and evaluation of Grant as a commander, although there is little in the way of new information or novel viewpoints. The work is marred by its slight documentation, and it lacks a thorough grounding in the enormous amount of Civil War literature or knowledge of the intricacies of the period.

Pennsylvania State University

WARREN W. HASSLER, JR.

THE PAPERS OF THOMAS JORDAN JARVIS. Volume I, 1869–1882. Edited by *W. Buck Yearns*. (Raleigh, N. C.: State Department of Archives and History. 1969. Pp. lx, 680. \$5.00.)

THE publication of this volume offers yet another example of the important but often unheralded work being done in archives throughout the country. It does not advance a new thesis or make radical reinterpretations, nor does it plow fresh ground. What it does rather is to provide the most valuable kind of service to scholars: the assembling, editing, and publishing of primary source documents. This has become common practice for major figures such as presidents, but happily the work is rapidly being extended to what are often mislabeled "secondary figures." The papers of these so-called lesser lights frequently comprise the richest lodes for historians to mine, and we desperately need more collections such as this one.

By any criteria Thomas Jordan Jarvis could not be called a figure of first-rank historical importance, but he is a significant representative of a much disputed era in American history. Born in Currituck County, North Carolina in 1836, Jarvis put himself through Randolph-Macon College before going off to war. His military career was unspectacular until the battle of Drewry's Bluff, where he received a wound that left his right arm permanently crippled. After Appomattox he drifted into politics and soon became one of the Democratic redeemers resisting Republican rule in the state. Elected speaker of the house after the Republican defeat in 1870, Jarvis stood squarely behind the Conservative program, which included the impeachment of former Governor Holden, the slashing of state expenditures, the probing of railroad frauds, and the outlawing of secret societies. An effective party man, Jarvis was elected lieutenant-governor behind Zebulon Vance in 1876. He became governor three years later when Vance ascended to the Senate and held the office until 1885. This first volume of his papers covers only the period through 1882.

What emerges from his papers is a familiar general portrait of the Conservative redeemer bent on driving out the rascal Republicans, re-establishing white rule, restoring a policy of fiscal retrenchment, and encouraging economic development. However, the portrait is etched in a myriad of fascinating and revealing details that will interest social, economic, and political historians. Short on color and long on integrity, Jarvis tried earnestly to implement the Conservative credo. In so doing he enmeshed himself in a predictable dilemma. A staunch advocate of education for both races, he once declared in 1880: "I have taken a bold stand for education and burnt all the bridges behind me." He also vigorously supported the state asylums and orphanages, better treatment for convicts, and numerous other humanitarian reforms. But his commitment to fiscal conservatism and low tax rates constantly undercut his good intentions. He continually asked for social service appropriations that exceeded the resources of his revenue base and, predictably, got only a small part of them. His integrity extended to economic development. He encouraged any worthwhile investment or project in the state and devoted a large portion of his time to securing a coherent railroad system for North Carolina. But he scrutinized the capitalists like a watchdog and was quick to defend the state's interests in any dispute. His strong sense of duty and honor deterred him from automatically currying the favor of business and promoters.

The editorial work by Professor Yearns is adequate though somewhat minimal; it could use a short explanation of his procedures and methods, especially in regard to the selection of materials. The biographical essay is informative, straightforward, and not overly illuminating.

University of Rhode Island

MAURY KLEIN

THE HENRY E. HUNTINGTON LIBRARY AND ART GALLERY: FROM ITS BEGINNINGS TO 1969. By *John E. Pomfret*. (San Marino, Calif.: Huntington Library. 1969. Pp. x, 241. \$8.50.)

MUSEUM OF FINE ARTS, BOSTON: A CENTENNIAL HISTORY. In two volumes. By *Walter Muir Whitehill*. (Cambridge, Mass.: Belknap Press of Harvard University Press. 1970. Pp. xi, 442; 444-888. \$25.00 the set.)

MERCHANTS AND MASTERPIECES: THE STORY OF THE METROPOLITAN MUSEUM OF ART. By *Calvin Tompkins*. (New York: E. P. Dutton and Company. 1970. Pp. 383. \$10.00.)

THE American Gilded age saw one of its greatest achievements in the creation and development of institutions dedicated to the education, refinement, and instructive amusement of the public. Libraries, museums, art galleries, and universities required the great fortunes of the post-Civil War era to become effective. Justifying their existence on the basis of the longstanding belief that taste, morality, and democratic opportunity were the outcome of private wealth and stewardship, these institutions were essentially elitist in origin and composition, and their programs reflected the tastes and prejudices of the class that contributed most to their development. At the same time, given the inexorable pressure of the democratic ideal that permeated their rhetoric if not their practice, these institutions eventually had to yield to the demand of the enlarged middle class that it be allowed to share in the special experiences that the institutions promised. By the close of World War II, the trend of institutional growth was in the direction of mass participation and a greater equality of cultural opportunity, but there is still a question as to whether the rhetoric of the founders—

“to give enjoyment to all classes . . . to elevate men by purifying the taste and acting upon the moral nature . . . to lead by the creation of a standard of taste . . . to improvement in all branches of industry”—has truly been fulfilled.

During the next decade, many of these institutions will be celebrating anniversaries, and we may expect a steady stream of histories, acknowledging the wisdom, generosity, and foresightedness of the men responsible for their founding. The three books at hand suggest some of the problems to be encountered. Foremost among them is the decision as to whether an author should be personally involved in the institution's life or be an outsider, for the question of “objectivity” is especially pertinent in official histories of institutions that have survived long enough and that have become wealthy enough to be considered “establishments.” The author who has been closely connected with the institution—its intramural conflicts, its personalities, its decision-making, the day-to-day details—has made it his world and can write about it intimately; on the other hand, he is probably incapable of viewing it as it appears in all its weaknesses and strengths to the surrounding community. His history, then, is most usually limited to a view of the institution as a separate organism, unaffected by the environment in which it is planted and to a great extent unresponsive to the world it is intended to serve.

Apart from these considerations, the individual who writes from a special inside knowledge frequently finds it difficult to omit what historically may not be important or to make judgments between the relevant and the irrelevant. Such, for instance, is the case with Pomfret's history of the Henry E. Huntington Library and Gallery of Art. Pomfret is the complete insider; he is full of admiration for the institution's founder, the various directors of research, the members of the board of trustees, and the many prominent scholars whom the library has served. Although he defers to chronology, Pomfret's presentation is primarily bibliographical, a listing of titles, authors, and dates of books published by the library's fellows and employees that collectively symbolize his view that the institution's major purpose was to encourage research. Funds, investments, and expenditures interest him; but philosophy, motivation, goals—the human elements that give life and vigor to institutions—are ignored, as are social considerations. His compendium is useful to the historian as an outline, but an interpretive history of the Huntington remains to be written.

Whitehill achieves more objectivity, but since he is the director of the Boston Athenaeum, the predecessor of the Museum of Fine Arts, a member of the board of trustees of the museum, and an affectionate admirer of Boston's Brahmins, he is emotionally caught up in the institution's greatness and in the generosity of the group that sustained it. His intimate knowledge of individual donors and curators provides him with numerous anecdotes, which he relates with grace and humor; but his year-by-year summaries of curators' reports, although presenting a useful guide through pounds of publications, is frequently tedious and unintegrated. Each accession is mentioned, each donor given proper credit; the background material for many of the donations is fascinating, but his suggestion that we “bathe rather than fish” in his work requires an endurance that many readers may not be capable of achieving.

Calvin Tomkins is the witty outsider who garnered anecdotes and reminiscences of those involved with the Metropolitan's development in order to weave a smooth and engrossing tale of life and art in a great metropolis. Tomkins does not hesitate to criticize or to interpret. His work, however, lacks the intimate knowledge of Whitehill or Pomfret of the way in which budgets work and investments are made—the kind of information useful to those concerned with the institution as a business entity;

and because of the absence of footnotes and bibliography, it is difficult to separate the apocryphal from the true in his narrative. Thus, although his gossip is charming—*New Yorker* style—and excellently integrated with the history of the institution, in the absence of corroborative evidence, the historian must take it with a grain of salt.

Some interesting conclusions may be drawn from these three volumes. In the first place, they reflect the inordinate emphasis Americans have always placed on education as a public good—indeed, as public salvation, substituting it for the church or state as keeper of the public morals. At times the Boston and New York museums were affected by the controversies between those who sought only esthetic experiences from art and those who regarded it as moral uplift and regeneration. But the educators always won.

Second, these histories reflect the problems raised by minority control in a mass culture. The “WASP-ish” nature of American cultural institutions clearly revealed in these books so removed these organizations from the mainstream of American social development that it has been difficult to get back into it, as the recent nod of the Metropolitan toward the black community of New York demonstrates—a well-intentioned but ignorant nod, so that the “Harlem On My Mind” exhibition resulted more in group antagonisms than in group reconciliations.

Even more, in the attempt of the modern museum to see that “quality and excellence are known more broadly,” as the present director of the Metropolitan insists should be the case, the question of numbers has posed problems of quality as well as of finances. Museum buildings are too small to accommodate the thousands of viewers who may come on a single day to view a popular exhibition; these find not quiet and serenity in the contemplation of esthetic objects, but dense, fast-moving crowds in which they are firmly and quickly pushed past objects whose beauty they must take on faith. The enormous number of beautiful objects, even if seen, must lead to cultural indigestion; certainly it could never lead to the development of positive feelings about art and ancient civilizations.

Finally, the tradition-oriented policy of American museums has tended to alienate the working artist striving for new expression. Artists have had to contend with establishments rather than draw inspiration from them, as it had originally been hoped. In the long run, such alienation may have proved healthy for some artists, but it has also meant that they have been cut off from nourishing traditions that would provide “the continuity of change” that Tomkins believes constituted the Metropolitan’s contribution. Revolution from the establishment—in art as in politics—takes its toll and is as wasteful as it is productive of change; moreover, the change that takes no account of tradition is not necessarily a change for the good.

It should be pointed out that all three of the institutions here discussed have come to recognize the problems inherent in their situation as elitist organizations dedicated to preserving and communicating the best that man has wrought, while serving a democracy that puts a premium on mass production and standardization of experiences. And all three institutions—as well as those not yet accounted for—are making liberal efforts to face up to the complexities of their position and to reconcile the seemingly irreconcilable elements.

University of Wisconsin, Milwaukee

LILLIAN B. MILLER

THE AMERICAN MYTH OF SUCCESS: FROM HORATIO ALGER TO NORMAN VINCENT PEALE. By *Richard Weiss*. (New York: Basic Books. 1969. Pp. 276. \$6.95.)

WEISS's book, a revision of his Columbia dissertation, is a confused and confusing attempt to analyze "the cluster of ideas" that constitute "the belief that all men, in accordance with certain rules, but exclusively by their own efforts, can make of their lives what they will." In an eight-part introductory statement, which appears to be a chapter précis, Weiss eschews any effort to determine whether or not this belief, at any stage of its development, reflects the empirical world. His concern is solely with the myth's content at various points in American history.

This approach—"intellectual analysis," the author states—partially explains the difficulties one has with the book. Intellectual analysis, or whatever, is not historical interpretation. Weiss's myth takes on a life of its own and its persistence (success?) in American life is explained by its adaptability to historical exigency. Like the Americans who are supposed to believe it, the myth is remarkably pragmatic: the moral homilies of the Puritan divines become the *Reader's Digest* platitudes of a Norman Vincent Peale. In describing the process of transition, Weiss relies on one of the principal elements of the myth itself, the belief that the American historical experience is unique. And, as the myth is pragmatic, so also is it unique. Weiss writes: "What does distinguish the American's pursuit of success [from the rest of mankind] is the particular significance he attaches to its achievement." In effect the historian of American mythology has become a mythologist.

Had Weiss paid more attention to the particular and concrete circumstances surrounding the various literary representations of his subject, he could have made a valuable contribution to our understanding of the nature of middle-class ideology. He would have seen, for example, that New Thought was something more than a revamping of the myth of success in the context of an industrial society. New Thought was a critique of middle-class ideology. Moreover, it developed its own structure to suit its purpose, and that purpose was nothing less than the pacification of the one class in America that had, at the time, the political and moral resources to change American society. What might have been I cannot say; but at a time when America's not yet vanquished middle classes were critically examining their social institutions and finding them wanting, New Thought writers, virtually all of them class-conscious, Eastern metropolitans, set out to neutralize the Protestant ethic's political potential. How well they succeeded can be measured by the circulation figures and the number of advertising pages in a current *Reader's Digest*.

American Historical Association

JOHN J. RUMBARGER

THE NEW COMMONWEALTH, 1877-1890. By *John A. Garraty*. [The New American Nation Series.] (New York: Harper and Row. 1968. Pp. xv, 364. Cloth \$7.95, paper \$2.25.)

INDUSTRIALISM affected nearly every aspect of late nineteenth-century American life. Its unifying tendencies encountered a stubborn localism and an individualistic national ethic that resisted social and economic planning. The resulting tensions explained the confusion that accompanied unprecedented growth and change. But organization and regulation arose both from necessity and design. The "greatly expanded reliance by individuals upon group activities" is the theme that Professor Garraty employs to analyze a nation that was sometimes unified against its will.

Garraty treats the era's major public activities, and his discussion of groups like Negroes, Indians, and some immigrants is judicious. He shows that wherever they were able to do so, farmers accepted the new technology as quickly as did businessmen. They also benefitted from an expanding educational system and special government assistance. But agrarians typified alternating desires to embrace and to reject the new benefits and demands of an unfamiliar industrial culture.

Garraty has done depth research in official documents to reveal labor's progress, grievances, and ambivalence toward the emerging national economy, whose need for efficiency and stability affected personal as well as public life. Garraty humanizes the familiar story of economic growth by showing how marketing techniques created national tastes and expanded the range of goods and services available to an increasing number of people.

The chapter on politics would be more stimulating if Garraty had better developed the theme of nationalization. The treatment is seldom perceptive, and the approach is too presidential. Politicians were concerned with establishing national coalitions and party loyalty. Voters seldom expected or desired the executive initiative that has produced both progress and error in this century. And the assertion that local, religious, and ethnic considerations affected voters more than national issues is debatable. Behind partisan debate over tariff protection, the currency, and internal improvements there were differing conceptions of how the nation should develop.

Garraty often seems uncomfortably poised between praising the era's obvious achievements and condemning its inevitable uneven development. But he has written a useful synthesis, and the book contains insights and information that will help readers seeking a new view of a complex period.

University of Texas, Austin

H. WAYNE MORGAN

IMMIGRANTS AND POLITICS: THE GERMANS OF NEBRASKA, 1880-1900. By *Frederick C. Luebke*. (Lincoln: University of Nebraska Press. 1969. Pp. viii, 220. \$7.95.)

PROFESSOR Luebke is the latest to subject the electoral politics of the late nineteenth century to quantitative analysis. His conclusions are familiar: Nebraska Germans normally voted along ethnic and especially religious lines, which made most of them Democrats. During the 1880's the pattern was etched deeper, as conservative Catholics and Missouri Synod Lutherans reacted, in the name of *die persönliche Freiheit*, against native American *Kaltwassermänner*, the *Weiberstimmrechts-Humbig*, and the *deutschfeindliche Schulzwangsgesetz*. On the other hand, German sectarians and Nebraska Synod Lutherans sympathized with the prohibitionists and Sabbatarians and, like them, tended to vote Republican. When economic reform displaced social reform in the 1890's, ethnic politics subsided, and Germans divided much as other voters did. For some, this involved a major shift of partisan loyalty. Since social conservatives were also fiscal conservatives, by 1896 many old Democrats had crossed over to the Republicans. By 1900 something like the old ethnic pattern was restored, reinforced by an upsurge of German cultural nationalism even as the cultural distinctiveness of the immigrant generation was passing away.

When Professor Luebke leaves electoral and demographic statistics for psychology, he is less convincing. In spite of his thorough combing of the local German press and the county histories, evidence for the two character types that he posits—assimilationist versus preservationist—is almost wholly lacking. Instead we are given, once

again, those fictional Dakota Norwegians, optimistic Per Hansa and forlorn Beret Holm, framed in sociological theories abstracted from Italian or Ukrainian experience in other parts of the country. The argument becomes tautological at times: to be assimilated is to be a Per Hansa, and Per Hansa's traits of character explain assimilation. Here and there Luebke unaccountably merges both types, and presumably all the churches, into "the German," with a common dislike of "the Yankee." The book demonstrates the limitations as well as the indispensability of quantitative analysis.

Washington University

ROWLAND BERTHOFF

NIGHT RIDERS OF REELFOOT LAKE. By *Paul J. Vanderwood*. (Memphis: Memphis State University Press. 1969. Pp. viii, 159. \$6.50.)

DURING the first decade of this century the attempt by the American Tobacco Company to control prices prompted the depressed tobacco farmers of the Tennessee-Kentucky border to organize as Night Riders. The populist-vigilante violence of their Black Patch War inspired, in the similarly depressed 1930's, a novel by Robert Penn Warren and two monographs (curiously, neither of them cited by the author of this one). Less well known, it also simultaneously inspired a distinct Night Rider movement in the northwestern corner of Tennessee, where the promoters of the West Tennessee Land Company, by threatening to drain Reelfoot Lake and to charge for fishing and timber rights, challenged the traditional freedom of the local fishermen and hill farmers. In response, the Reelfoot Night Riders launched a reign of terror during the warm months of 1908 that culminated in the kidnapping of two prominent attorneys representing the promoters, the lynching of one of them, and the attempted murder of the other.

The Reelfoot violence quickly became an issue in Tennessee politics and led to the calling out of the militia, the arrest of scores of alleged Night Riders, the conviction of eight for murder and their acquittal in appeal on technicalities, and finally to the state's acquisition of Reelfoot Lake in 1914 as a public park and preserve. In constructing this slim but careful monograph, Paul Vanderwood has relied heavily on newspapers and oral history, having conducted over seventy interviews during the middle 1950's. His reconstruction of the intricacies of the trials in the absence of the transcripts, which had been burned in the Obion County Courthouse, is particularly commendable, as is his suggestive analysis of the backgrounds and motives of the Night Riders. Although an able narrative, it lacks the theoretical focus and quantitative rigor of, for instance, Leonard Richards' recent analysis of anti-abolitionist mob violence, *Gentlemen of Property and Standing*.

The Johns Hopkins University

HUGH DAVIS GRAHAM

HEIR TO EMPIRE: UNITED STATES ECONOMIC DIPLOMACY, 1916-1923.

By *Carl P. Parrini*. ([Pittsburgh:] University of Pittsburgh Press. 1969. Pp. x, 303. \$8.95.)

PROFESSOR Parrini's valuable study presents essentially an economic analysis of business and related forces that influenced both domestic and foreign policies of the United States government from 1916 to 1923 and after. Parrini offers persuasive evidence that the early 1920's do not really represent an isolationist era.

The Harding, Hoover, Hughes, Mellon economic policies are presented as making

no sharp break with the earlier Wilson policies but reflecting much continuity. While bankers nagged the government to guarantee risky capital investments during these years, political leaders generally refused to subsidize such ventures on the reasonable grounds that profits would go to corporations and not to taxpayers. American statesmen of this period tended to believe that disarmament and the open door were the twin keys to peace and community. In a disarmed world the United States would be better able to exercise its expanding economic influence and power.

Parrini recognizes that a major question facing American leaders in this period was whether world markets could be expanded any further without reconstruction of the European economy. This dovetailed with United States government policy favoring reduction of German reparations and the readmission of Germany to the world economy under less onerous conditions than those imposed by the Versailles Treaty. The chief American tactic to bring this about was to refuse to lighten the war debts owed by the Allies until the latter reduced Germany's burden.

Trouble developed early between the United States and the Allies over Franco-British tendencies to violate the open door principle in the mandated areas. Hoover, as secretary of commerce, insisted that reconstruction be based on the open door. The State and Commerce Departments worked together for this end. President Harding granted to Hoover the virtual right to veto foreign policy decisions in economic areas.

Parrini's concentration on economic forces in diplomatic history tends occasionally to subordinate major political developments. Like most historians, he neglects the Franco-Belgian occupation of the Ruhr with its crucial economic and political implications for both the American and European economies. Both the Dawes papers and the Hoover papers are major sources here.

Elmhurst College

ROYAL J. SCHMIDT

MAKERS, USERS, AND MASTERS. By *Arthur F. Bentley*. Edited with an introduction by *Sidney Ratner*, assisted by *Peter Asch*. ([Syracuse, N. Y.:] Syracuse University Press. 1969. Pp. xxviii, 290. \$8.75.)

ARTHUR F. Bentley (1870-1957), American political scientist, economist, philosopher, and editorialist (for the *Chicago Times-Herald* and *Record-Herald*), wrote *Makers, Users, and Masters* a half-century ago. Thanks to the efforts of Professor Ratner, his long-time friend, the manuscript has become a book with twenty-four brief, rewarding chapters. Basing most of his comments on material published by Congressional committees, federal regulatory agencies (especially the Federal Trade Commission and the Interstate Commerce Commission), cabinet departments (Labor and Interior), the US Commission on Industrial Relations of 1912-15, Louis D. Brandeis, Thorstein Veblen, and other significant contemporary sources, Bentley portrayed the American economy of the post-World War I era in a somber light. At the close of the Wilson administration, Bentley believed that giant business corporations, the "Masters" of the title, dangerously threatened the political and economic freedom of lesser enterprises, of industrial workers, and of farmers. He also maintained the existence of a competitive relationship between the nation's democratic political government and its autocratic industrial leadership and that, as the country could not endure half democratic and half autocratic, a momentous confrontation was in the offing. Events such as the Bolshevik Revolution of 1917, the widespread labor-management strife of 1919, and the persistent suppression of the civil rights of radicals and socialists by state and national governments did not help dispel Bentley's gloom. Still, he thought

that the Scylla of industrial autocracy and the Charybdis of convulsive proletarian revolution might be avoided in proper, civilized fashion; and toward this end he urged an alliance between middle-class consumers (the "Users") and workers and farmers (the "Makers").

As a prelude to his solutions for setting American society aright, Bentley analyzed and stringently criticized such undesirable economic phenomena as oligopoly and monopoly, profiteering (which he deemed an unproductive, appropriative extension of legitimate profit-taking), and waste (by which he specifically meant advertising and the countless and needless duplications of railroad lines, factories, public service plants, and retail stores). To eliminate or at least ameliorate these evils, Bentley asked his future "Makers and Users" coalition to take advantage of the federal government's police, taxing, and legislative powers and to make it easier to democratize industrial government and redistribute the national wealth by allowing for "just" compensation. Should these attempts prove abortive or fall short of final realization, he suggested other economic weapons against the Masters—publicly owned competitive enterprises, a general strike by the Makers, and a general boycott by the Users.

Students of the Progressive movement will find this primary source a welcome addition to the literature of the period. They will also profit from Ratner's valuable introduction.

Indiana University

IRVING KATZ

IDEOLOGIES AND UTOPIAS: THE IMPACT OF THE NEW DEAL ON AMERICAN THOUGHT. By *Arthur A. Ekirch, Jr.* (Chicago: Quadrangle Books. 1969. Pp. ix, 307. \$8.50.)

THE Great Depression, writes Arthur Ekirch, shattered the already shaken faith of liberal intellectuals in the nation's capacity to sustain abundance, progress, and equal opportunity without a massive intervention of the state or a permanent commitment to a nationally planned economy. Their proposals for a new public philosophy came to life in the New Deal, in Wallace's AAA, in Tugwell's NRA, in the TVA of Norris, Morgan, and Lilienthal, in the structurally more subtle notions of Arnold and Brandeis, and finally in proto-Keynesian policies of public spending, first in public works and welfare, finally in preparation for war. Intellectual support for the New Deal nevertheless remained partial and episodic. Roosevelt's indiscriminate receptiveness to all new ideas and his instinct for ideological compromises drew fire from every quarter. Center-Progressives like Hoover and Left-Progressives like Lippmann feared statism and the loss of procedural rights. Marxists regretted bitterly the New Deal's commitment to state capitalism and the principle of favors for every economic group regardless of need. Right-wing radicals demanded welfare measures according to a basically Populistic scheme of priorities. Finally, with the collapse of peace in Europe, New Dealers and their intellectual critics alike were caught in the dilemma of wanting to support collective security without risk of war for the United States. A confused people engaged in a divisive debate over neutrality and preparedness that ended only with Pearl Harbor. The consequences, concludes Ekirch, were tragic and ironic. War intensified and made permanent the New Deal's program of national economic control, a Keynesian enthusiasm for public spending to compensate for deficiencies in the private economic system, and a commitment to a welfare program tied to a policy of permanent military preparedness. War encouraged intellectuals to dream of an "American Century," a New Deal for the entire world. In destroying fascism the

nation gained security for itself and its allies but gave up a degree of equality, justice, and individual liberty, a portion of its own democratic ideals. Because of war and the revolutionary nature of the modern world, the principal surviving legacies of the New Deal for the American people are nationalism and a corresponding impairment of freedom.

Professor Ekirch's book offers an authoritative and reasonably detailed description of the impact of the Depression, the New Deal, and the problem of war on some prominent segments of the intellectual community between 1930 and 1945. Despite its title, it focuses very little on ideologies and not at all on utopias. It is a study not so much of the impact of the New Deal on American thought as of the way a few familiar writers and thinkers viewed public affairs and interpreted public policy during the New Deal era. The author is familiar with their writings and has ranged widely in public accounts and private collections. Historians who have read chapters bearing on the same subject in books by Ralph Gabriel, Richard Hofstadter, and Arthur Schlesinger, Jr., will find little here that is not familiar, save for the innuendo that somehow the New Deal is to blame for the garrison state we now live in, and the contradictory view that the New Deal was itself an inevitable consequence of the Depression and of a world-wide move toward statism in the twentieth century. The story Professor Ekirch tells seems to me to deserve a more searching and less simplistic and melancholy judgment than the one he bestows on it.

University of Washington

OTIS A. PEASE

A UNION OF INDIVIDUALS: THE FORMATION OF THE AMERICAN NEWSPAPER GUILD, 1933-1936. By *Daniel J. Leab*. (New York: Columbia University Press. 1970. Pp. vi, 362. \$10.00.)

"The American Newspaper Guild," Walter Galenson has written, "merits a distinct place in a general history of unionism because of its success in organizing a group of workers who would have seemed to be very poor union material from an objective viewpoint." The record of this union takes on particular significance at a time when the number of white-collar workers exceeds the number of blue collar, when the professional workers among the white-collar ranks are rapidly increasing, and when the future of the labor movement rests, to a degree, on its ability to appeal to other than manual workers.

The general outlines of the history of the ANG are familiar enough, but Daniel Leab is the first scholar to have provided a full-scale and exhaustively researched account of the early years of the organization, from its founding in 1933 to its affiliation with the American Federation of Labor in 1936. Created as the result of the impact of the National Industrial Recovery Act, the ANG was at the outset more a professional association than a trade union. The failure of the National Recovery Administration to improve the working conditions of newspaper reporters, the ineffectiveness of the Newspaper Industrial Board, and the resistance to collective bargaining on the part of the newspaper publishers caused the organization increasingly to take on the character of a trade union, and by the time it decided to affiliate with the AFL it had become "a labor union . . . in fact if not altogether in theory." It had accomplished little in the way of economic gains for its reporter members—it was still a craft union in 1936—but it had survived. For this achievement, Mr. Leab gives the lion's share of the credit to Heywood Broun. As Irving Bernstein has observed, Broun was "the oddest union leader in American labor history," but the colorful columnist never comes alive in Leab's pages.

The principal shortcoming of Leab's book is the author's failure to place his subject in a sufficiently broad context. He is too little concerned with the implications of his story for an understanding of the NRA experience, the labor movement of the 1930's, and the factors that make for success or failure in the area of white-collar unionism. He has, nevertheless, provided us with a wealth of information about the initial years of a small but unusual union and, as was his major purpose, has described the process by which an organization that started out as a professional association became a militant trade union.

University of Michigan, Ann Arbor

SIDNEY FINE

EL MARQUESADO DEL VALLE: TRES SIGLOS DE RÉGIMEN SEÑORIAL EN NUEVA ESPAÑA. By *Bernardo García Martínez*. [Centro de Estudios Históricos, New Series, Number 5.] ([México, D. F.:] Colegio de México. 1969. Pp. xiv, 175.)

In this pioneering study Sr. García Martínez has undertaken the complicated task of tracing the judicial development of the Marquesado del Valle from 1529, when Hernán Cortés was empowered to create a *mayorazgo*, or entailed estate with *señorío* over 23,000 vassals, to 1811, when the Cortes Generales abolished tribute and señorial jurisdiction.

Drawing primarily upon selected *legajos* in the rich Hospital de Jesús section (the former archive of the marquesado and the hospital) of the Archivo General de la Nación, Mexico, and published collections of documents, García concludes that the marquesado was in fact a limited *señorío*, characterized by judicial prerogatives and the right of eminent domain, and as such was almost unique in Spanish American history. The data he presents are a useful introduction to his subject and an addition to our knowledge of the political and economic history of New Spain. But in view of the almost incredible amount of unexploited documentation in Mexico and Spain, much remains to be done, as García himself emphatically points out.

University of New Mexico

ELEANOR B. ADAMS

* * * Other Books Received * * *

General

- ABEL, ANNIE HELOISE, and KLINGBERG, FRANK J. (ed. with introd. and notes). *A Side-Light on Anglo-American Relations, 1839-1858: Furnished by the Correspondence of Lewis Tappan and Others with the British and Foreign Anti-Slavery Society*. Reprint; New York: Augustus M. Kelly. 1970. Pp. vi, 407. \$12.50.
- AYLING, S. E. *Nineteenth-Century Gallery: Portraits of Power and Rebellion*. New York: Barnes and Noble. 1970. Pp. 454. Cloth \$8.50, paper \$3.95.
- BARTLETT, ROLAND W. (comp.). *The Success of Modern Private Enterprise*. Danville, Ill.: Interstate Printers and Publishers. 1970. Pp. 281. \$5.95.
- Bibliography of the History of Medicine*. No. 4, 1968. National Library of Medicine. U.S. Department of Health, Education, and Welfare, Public Health Service, National Institutes of Health. Public Health Service Publication No. 1540-4. Bethesda, Md.: National Library of Medicine. [1970.] Pp. vi, 299. \$2.75.
- BIRNBAUM, KARL E. *Peace in Europe: East-West Relations 1966-1968 and the Prospects for a European Settlement*. Written under the auspices of the Harvard Center for International Affairs and published in co-operation with the Royal Institute of International Affairs. New York: Oxford University Press. 1970. Pp. xii, 159. \$2.50.
- BLOOMFIELD, MORTON W. *Essays and Explorations: Studies in Ideas, Language, and Literature*. Cambridge, Mass.: Harvard University Press. 1970. Pp. x, 321. \$8.50.
- BLUM, JEROME, et al. *The European World: A History*. 2d ed.; Boston: Little, Brown. 1970. Pp. xxii, 1219. 1-vol. ed. \$10.95, 2-vol. ed. \$7.95 each.
- BRAUDEL, FERNAND. *Écrits sur l'histoire*. Science de l'histoire. [Paris:] Flammarion. 1969. Pp. 314.
- BRUNET, JEAN-PAUL, and PLESSIS, ALAIN. *Explications de textes historiques: De la Révolution au XX^e siècle*. Collection U2, Ser. "Histoire contemporaine." Paris: Armand Colin. 1970. Pp. 493.
- CHARLOT, MONICA. *La persuasion politique*. Dossiers, U2. Paris: Armand Colin. 1970. Pp. 168.
- CLISSOLD, STEPHEN (ed.). *Soviet Relations with Latin America, 1918-1968: A Documentary Survey*. Issued under the auspices of the Royal Institute of International Affairs. New York: Oxford University Press. 1970. Pp. xx, 313. \$12.00.
- CURTIS, MICHAEL (ed.). *Marxism*. New York: Atherton Press. 1970. Pp. 336. \$6.95.
- DE LA MAHOTIÈRE, STUART. *Towards One Europe*. Pelican Original. [Baltimore:] Penguin Books. 1970. Pp. 331. \$1.95.
- DOMMANGET, MAURICE. *Les grands socialistes et l'éducation: De Platon à Lénine*. Collection U. Paris: Armand Colin. 1970. Pp. 469.
- DOUGLAS, MARY. *Natural Symbols: Explorations in Cosmology*. New York: Pantheon Books. 1970. Pp. xvii, 177. \$5.95.
- EVERETT, ALEXANDER H. *New Ideas on Population with Remarks on the Theories of Malthus and Godwin*. With the addition of the correspondence between Everett and George Tucker on the Malthusian Theory, published in *The Democratic Review* for 1845 & 1847. Reprints of Economic Classics. Reprint of 2d ed. (1826); New York: Augustus M. Kelley. 1970. Pp. xxii, x, 12-410. \$10.00.
- FINKELSTEIN, LOUIS (ed.). *The Jews: Their History*. Schocken Paperbacks on Jewish Life and Religion. 4th ed.; New York: Schocken Books. 1970. Pp. xiv, 556. \$3.95.
- GATHORNE-HARDY, G. M. (tr. and discussed.) *The Norse Discoverers of America: The Wine-land Sagas*. With a new preface by the Author and new introd. by GWYN JONES. Reprint; New York: Oxford University Press. 1970. Pp. xxv, 304. \$9.00. See rev. of 1st ed. (1921), *AHR*, XXVII (Jan. 1922), 325.
- Historical Atlas of the World*. New York: Barnes and Noble. 1970. Pp. unnumbered, 108 maps. Cloth \$4.50, paper \$2.95.
- HOFFMAN, ROBERT (ed.). *Anarchism*. Atherton Controversy. New York: Atherton Press. 1970. Pp. 165. Cloth \$6.95, paper \$2.95.
- HOPE, MARJORIE. *Youth against the World*. Boston: Little, Brown. 1970. Pp. xiv, 409. \$7.95.
- HORGAN, PAUL. *The Heroic Triad: Essays in the Social Energies of Three Southwestern Cultures*. New York: Holt, Rinehart and Winston. 1970. Pp. xii, 256. \$7.95.
- JERVIS, ROBERT. *The Logic of Images in International Relations*. Written under the auspices of the Center for International Affairs, Harvard University. [Princeton:] Princeton University Press. 1970. Pp. xi, 281. \$8.50.
- JOHNSON, ELMER D. *History of Libraries in the Western World*. 2d ed.; Metuchen, N. J.: Scarecrow Press. 1970. Pp. vi, 7-521. \$12.50.
- KEMP, TOM. *Industrialization in Nineteenth-Century Europe*. New York: Humanities Press. 1969. Pp. ix, 230. Cloth \$8.00, paper \$4.00.
- MAHN-LOT, MARIANNE. *La découverte de l'Amérique*. Questions d'histoire. [Paris:] Flammarion. 1970. Pp. 142.
- MERKES, MANFRED. *Die deutsche Politik im spanischen Bürgerkrieg, 1936-1939*. Bonner historische Forschungen, No. 18. 2d rev. ed.; Bonn: Ludwig Röhrscheid Verlag. 1969. Pp. 477. DM 58. See rev. of 1st ed. (1961), *AHR*, LXVII (July 1962), 1107.

► Books listed were received by the *AHR* between June 1 and August 1, 1970. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

MERRIAM, CHARLES E. *New Aspects of Politics*. With a foreword by BARRY D. KARL. 3d rev. ed.; Chicago: University of Chicago Press. 1970. Pp. 359. \$9.50.

Race Relations in the USA, 1954-68. Keesing's Research Report. New York: Charles Scribner's Sons. 1970. Pp. viii, 280. Cloth \$6.95, paper \$2.95.

RODWIN, LLOYD. *Nations and Cities: A Comparison of Strategies for Urban Growth*. Boston: Houghton Mifflin. 1970. Pp. xvi, 395. \$7.95.

ROMANO, RICHARD, and LEIMAN, MELVIN. *Views on Capitalism*. Beverly Hills, Calif.: Glencoe Press. 1970. Pp. x, 449.

SELLARS, ROY WOOD. *Social Patterns and Political Horizons*. Nashville: Aurora Publishers. 1970. Pp. xii, 408. \$10.00.

STAVRIANOS, L. S. *The World to 1500: A Global History*. Englewood Cliffs, N. J.: Prentice-Hall. 1970. Pp. 399.

STEBBINS, RICHARD P., and AMOIA, ALBA (eds.). *Political Handbook and Atlas of the World, 1970: Governments and Intergovernmental Organizations as of September 1, 1969, with Supplementary Data through January 1, 1970*. New York: Simon and Schuster for the Council on Foreign Relations. 1970. Pp. x, 534, 48 maps. \$19.95.

WEIL, GORDON L., and DAVIDSON, IAN. *The Gold War: The Story of the World's Monetary Crisis*. New York: Holt, Rinehart and Winston. 1970. Pp. vii, 245. \$6.95.

WELD, ISAAC, JUNIOR. *Travels through the States of North America and the Provinces of Upper & Lower Canada during the Years 1795, 1796 & 1797*. In 2 vols. America through European Eyes. Reprint of 4th ed. (1807); New York: Augustus M. Kelley. 1970. Pp. xix, 427; viii, 376. \$27.50 the set.

WORSLEY, PETER. *The Third World*. The Nature of Human Society Ser. 2d ed.; [Chicago:] University of Chicago Press. 1970. Pp. xvi, 373. \$7.50.

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AUSTIN, M. M. *Greece and Egypt in the Archaic Age*. Proceedings of the Cambridge Philological Society, Supplement No. 2. Cambridge: the Society. 1970. Pp. 75. 21s.

MESLIN, MICHEL. *Le christianisme dans l'empire romain*. Collection SUP. "L'historien," No. 4. Paris: Presses Universitaires de France. 1970. Pp. 195. 10 fr.

PINGREE, DAVID. *Census of the Exact Sciences in Sanskrit*. Ser. A, Vol. I. Memoirs of the American Philosophical Society, Vol. LXXXI. Philadelphia: the Society. 1970. Pp. vii, 60. \$5.00.

STOCKTON, DAVID (selected and ed. with introd. and notes). *Thirty-five Letters of Cicero*. New York: Oxford University Press. 1969. Pp. xxxii, 241. \$2.10

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ANDREWES, PATIENCE. *Frederick II of Hohenstaufen*. The Clarendon Biographies. New York: Oxford University Press. 1970. Pp. 64. \$1.60.

DAVIES, COLIN. *The Emergence of Western*

Society: European and English History 300-1200. The Evolution of Western Society. New York: Humanities Press. 1970. Pp. 403. Cloth \$6.00, paper \$4.50.

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FORD, GORDON B., JR. (tr. from the Latin with an introd.). *The Letters of St. Isidore of Seville*. 2d rev. ed.; Amsterdam: Adolf M. Hakkert. 1970. Pp. 69, 16 pls.

FULCHER OF CHARTRES. *A History of the Expedition to Jerusalem, 1095-1127*. Tr. by FRANCES RITA RYAN (SISTERS OF ST. JOSEPH). Ed. with an introd. by HAROLD S. FINK. Knoxville: University of Tennessee Press. 1969. Pp. xiv, 348. \$13.50.

GALBRAITH, V. H. (ed.). *The Anonimale Chronicle, 1333 to 1381: From a MS. Written at St. Mary's Abbey, York*. Reprint; New York: Barnes and Noble. 1970. Pp. xlix, 216. \$9.50.

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JONES, THOMAS M. (ed.). *The Becket Controversy*. Major Issues in History. New York: John Wiley and Sons. 1970. Pp. viii, 165. Cloth \$5.95, paper \$3.50.

KIMBALL, ELISABETH G. (ed.). *Sessions of the Peace for Bedfordshire, 1355-1359, 1363-1364*. Historical Manuscripts Commission, JP16. London: H.M. Stationery Office; distrib. by British Information Services, New York. 1969. Pp. viii, 145. \$11.70 postpaid.

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STÜDELL, BERNHARD E. J. *Minoritenniederlassungen und mittelalterliche Stadt: Beiträge zur Bedeutung von Minoriten- und anderen Mendikantenanlagen im öffentlichen Leben der mittelalterlichen Stadtgemeinde, insbesondere der deutschen Schweiz*. Franziskanische Forschungen, No. 21. Werl, Westf.: Dietrich-Coelde-Verlag. 1969. Pp. 134. DM 26.

William of St Thierry, The Works of. Vol. II, *Exposition on the Song of Songs*. Tr. by MOTHER COLUMBA HART OSB. Introd. by J. M. DÉCHANET OSB. Cistercian Fathers Ser., No. 6. Spencer, Mass.: Cistercian Publications. 1970. Pp. xlviii, 169. \$7.50.

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ARMYTAGE, W. H. G. *Four Hundred Years of English Education*. 2d ed.; New York: Cambridge University Press. 1970. Pp. xii, 353. Cloth \$7.00, paper \$3.45.

BRIGGS, ASA. *Victorian People: A Reassessment of Persons and Themes, 1851-67*. Rev. ed.; [Chicago:] University of Chicago Press. 1970. Pp.

ix, 312. \$7.50. See rev. of 1st ed. (1955), *AHR*, LXI (Apr. 1956), 694.

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Canada. OECD Economic Surveys. [Washington:] Organisation for Economic Co-operation and Development. 1970. Pp. 50. 80 cents.

CANNON, JOHN. *Lord North: The Noble Lord in the Blue Ribbon*. General Ser. No. 74. London: Historical Association. 1970. Pp. 29. 5s.

The Churchill Statue at the House of Commons: The Unveiling of a Memorial Statue to Sir Winston Churchill at the House of Commons, 1st December 1969, by Lady Spencer-Churchill. London: H.M. Stationery Office; distrib. by British Information Services, New York. 1970. Pp. 9. 60 cents, postpaid.

FINLAYSON, GEOFFREY, B. A. M. *Decade of Reform: England in the Eighteen Thirties*. Foundations of Modern History. New York: W. W. Norton. 1970. Pp. ix, 115. \$5.00.

GALE, RICHARD. *The Worcestershire Regiment (The 29th and 36th Regiments of Foot)*. Famous Regiments. New York: Hillary House. 1970. Pp. 122. \$5.25.

GARRISON, CHESTER A. *A Map of Shakespeare's London*. Corvallis: Oregon State University Press. 1970. 35 cents.

HIBBERT, CHRISTOPHER (ed. and introd.). *Recollections of Rifleman Harris, as Told to Henry Curling*. [Hamden, Conn.]: Archon Books. 1970. Pp. ix, 128. \$5.50.

HILLS, R. J. T. *The Royal Horse Guards (The Blues)*. Famous Regiments. New York: Hillary House. 1970. Pp. 117. \$5.25.

JACKSON, DONALD. *Intermarriage in Ireland 1550-1650*. Minneapolis: Cultural and Educational Productions. 1970. Pp. 84.

KALLICH, MARTIN. *The Other End of the Egg: Religious Satire in Gulliver's Travels*. Studies in British History and Culture. Vol. III. [Bridgeport, Conn.]: Conference on British Studies at the University of Bridgeport. 1970. Pp. ix, 119.

LASSEK, ARTHUR M. *The Unique Legacy of Doctor Hughlings Jackson*. Springfield, Ill.: Charles C Thomas. 1970. Pp. v, 146. \$6.75.

MACLYSAGHT, EDWARD. *Irish Life in the Seventeenth Century*. Reprint; New York: Barnes and Noble. 1969. Pp. 480. \$17.50. See rev. of 1st ed. (1939), *AHR*, XLVI (Oct. 1940), 204.

MCCAUGHT, KENNETH. *The History of Canada*. New York: Praeger. 1970. Pp. 336. \$7.50.

MERTON, ROBERT K. *Science, Technology & Society in Seventeenth Century England*. Reprint; New York: Howard Fertig. 1970. Pp. xxxii, 279. \$11.00.

MORRELL, W. P. *British Overseas Expansion and the History of the Commonwealth: A Select Bibliography*. Helps for Students of History, No. 63. 2d rev. ed.; [London:] Historical Association. 1970. Pp. 48. 5s.

NAIDIS, MARK. *The Second British Empire, 1783-1965: A Short History*. Reading, Mass.: Addison-Wesley. 1970. Pp. xii, 273. \$7.50.

National Maritime Museum. Catalogue of the Library. Vol. II, Biography. In 2 parts. London:

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NORTON, J. E. *A Bibliography of the Works of Edward Gibbon*. Reprint; New York: Oxford University Press. 1970. Pp. xvi, 256. \$9.95.

SAINTY, J. C. *Lieutenants of Counties, 1585-1642*. Bulletin of the Institute of Historical Research, Special Supplement No. 8. [London:] University of London, Athlone Press. 1970. Pp. vi, 46. £1.

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WATSON, ANDREW G. *The Manuscripts of Henry Savile of Banke*. London: Bibliographical Society; distrib. by Oxford University Press, New York. 1969. Pp. xi, 102. \$8.50.

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France

ANGEVILLE, A. D'. *Essai sur la statistique de La population française: Considérée sous quelques-uns de ses rapports physiques et moraux*. With an introd. by EMMANUEL LE ROY LADURIE. Maison des Sciences de l'Homme. Rééditions, No. 6. The Hague: Éditions Mouton. 1969. Pp. xxxix, 367, xxxiv. 84.50 gls.

ARTOM, GUIDO. *Napoleon Is Dead in Russia*. Tr. from the Italian by MURIEL GRINDROD. New York: Atheneum. 1970. Pp. 256. \$5.95.

BAGUENARD, JACQUES, et al. *Le Président de la V^e République*. Introd. by JACQUES GEORGE. Dossiers, U-2. Paris: Armand Colin. 1970. Pp. 111.

COTTRELL, ROBERT D. *Brantôme: The Writer as Portraitist of His Age*. Études de philologie et d'histoire, No. 15. Geneva: Librairie Droz. 1970. Pp. 169.

DAUMARD, ADELINE. *Les bourgeois de Paris au XIX^e siècle*. Science de l'histoire. [Paris:] Flammarion. 1970. Pp. 382.

DROZ, JACQUES. *De la Restauration à la Révolution, 1815-1848*. Collection U2. Paris: Armand Colin. 1970. Pp. 287.

GOUBERT, P. (director). *Annales de démographie historique, 1969 (Études, chronique, documents, bibliographie)*. Villes et villages de l'ancienne France. Ed. in Chief: J. DUPAQUIER. Paris: Éditions Sirey. 1970. Pp. 520. 50 fr.

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Communications

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TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

I enjoyed Anne Firor Scott's spirited review of my book, *Everyone Was Brave* (AHR, LXXV [1970], 1527), but I must correct her on one point. She writes: "His research was guided by an a priori judgment that American feminism failed. . . ." On the contrary, my preface states: "When I began studying the history of women it was with the usual assumption that the feminist movement had been at least moderately successful." It was only after years of work that I came to think otherwise, a conclusion I was all the more reluctant to accept as it required me to rewrite a complete first draft and throw out much archival material that was no longer pertinent to my inquiry, though it was of much interest to me personally and, obviously, would also have been to Professor Scott. All the same, I am glad to have stimulated her to "work harder and do better."

University of Wisconsin

WILLIAM L. O'NEILL

The following exchange of letters has been received in connection with the publication of Les K. Adler and Thomas G. Paterson, "Red Fascism: The Merger of Nazi Germany and Soviet Russia in the American Image of Totalitarianism, 1930's-1950's," AHR, LXXV (1970), 1046-64.

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

Les Adler and Thomas Paterson merit praise for compiling a massive collection of hard-line cold-war rhetoric in their article on "Red Fascism," but the thrust of the article is unclear if not misleading. They make no effort to evaluate how much influence such rhetoric had on the shaping of public opinion, and they seem to suggest that it was a cause rather than a result of the cold war. President Truman and the men who assisted him in formulating postwar foreign policy (Acheson, Marshall, Clayton, Clifford, Forrestal, Kennan, Lovett, Harriman, and the rest) were men of intelligence, ability, and long public experience who did not think in blind ideological stereotypes. There is enough material now becoming available for the authors to examine how and why post-war American foreign policy came to be made, if such is their purpose.

Furthermore, several errors of fact and interpretation seriously detract from the reliability of their account. First, they observe (on page 1055) that the Greek civil war "in reality" was "a struggle of Greeks against a British-supported monarchy with little interference by the Soviet Union." The Greek civil war, "in reality," was a conflict between two Greek factions: an armed Communist movement (the EAM/ELAS) and an unhappy non-Communist group whose members ranged from liberals and republicans (George Papandreou was premier for part of the civil-war period) to monarchists of various shades of reaction. Nor was British support of her wartime ally in any way out of order, and it should be noted that in 1944 and 1945 the EAM had agreed to accept the post-liberation Greek government. The Soviet Union may not have given the EAM direct support, but Albania, Yugoslavia, and Bulgaria certainly did, and the Soviet Union did carry on a diplomatic and propaganda campaign against the British

and the Greek government in the United Nations and elsewhere, did support Bulgarian territorial claims against Greece, did not object to the activities of its Balkan satellites, and even, early in 1946, approached the Greek government with the suggestion that it cede to the Soviet Union an island in the Dodecanese for a naval base. Nor did it lose interest in the Greek Communists until their cause was lost. Perhaps the *Foreign Relations* volume for Greece in 1946 was not available to the authors, but United Nations documents and press accounts have been available for years. Moreover, it appears to be poor scholarship for the authors to cite Richard Barnet's *Intervention and Revolution* as their sole reference in this matter when useful works on the Greek civil war by George Kousoulas, Edgar O'Ballance, and Stephen Xydis have long been at hand.

On page 1056 the authors state that "It was assumed, without understanding the Soviet security concerns or its national interest, that Russia was simply replacing Germany as the disrupter of peace in Europe." It does not follow from this simplistic view of Soviet policy, however, that either Soviet security concerns or national interest was served by the imposition of Communist regimes. Under non-Communist governments, Czechoslovakia willingly and Finland by peace treaty made far-reaching political and territorial concessions to the Soviet Union. Nevertheless, the former was ruthlessly Communized and the latter spared only by a narrow margin. As for the other East European nations liberated by the Red Army, Communization was again hardly called for on the grounds of security. With Germany crushed, these countries, whatever their form of government, were dependent on the Soviet Union for survival.

Further, the matter was not merely one of letting the Soviet Union have its own way in Eastern Europe. The Soviet Union did make territorial demands upon Turkey and interfere in the internal affairs of Iran in a most blatant manner; it did seek to undermine four-power control of Germany and force the West from Berlin (hardly in the Soviet "national interest" since it united the Western powers against the USSR and promoted the creation of the German Federal Republic); it did prevent Poland and Czechoslovakia from participating in the Marshall Plan; and it did send its satellite armies in North Korea marching southward in 1950. All this was conducted against a background of violent anti-Western propaganda and the intrigues and disorders fomented by local Communist parties that slavishly proclaimed their obedience to Moscow.

Before the authors reject ideology as a basis for Soviet action, they might recall recent events in Czechoslovakia and stretch their memories back to the wars of the French Revolution. At that time Russian armies operated in Belgium, Switzerland, and northern Italy. In the campaign against Napoleon, another army operated throughout Germany and was led by Czar Alexander into Paris. Yet these armies departed, and the results of their presence were minimal. The case was hardly the same with the Red Army during World War II.

Finally, the authors seem to ignore the fact that, however shrill the rhetoric, those who compared the Soviet Union with Nazi Germany were striking at a basic truth. However the two states differed in form, orientation, and personnel, as well as length of time in existence, both shared the common aim of subordinating the individual to an all-powerful state, and both possessed the machinery to carry out that aim. To someone opposed to such an aim, or to a victim of it, the difference between Stalin's Russia or Hitler's Germany was about the same as the difference between being drowned or buried alive.

Arlington, Virginia

DAVID F. RUDGERS

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

In the April issue of the *AHR* a study appeared on "Red Fascism" in America that left at least two of its readers transfixed with wonder. The essay claims to be treating the genealogy of an illusion, but it does not demonstrate that what it explores is indeed illusory. Is the analogy between Soviet and Nazi regimes, which scholars as esteemed as Carl Friedrich and Hannah Arendt have constructed, simply the result of self-delusion? Messrs. Paterson and Adler seem so convinced of this fact that they do not even deign to weigh arguments to the contrary. They prefer to belabor a whole generation of statesmen and educators for having committed an inexcusable blunder; that is, to have identified Stalin with Hitler merely because both used terror against political opponents, exterminated ethnic minorities, and trampled upon elemental human rights.

Such a parallel, we are told, is both specious and misleading. Stalin, while sometimes "idiosyncratic and brutal" in his methods, was basically a Marxist, whereas Hitler, as everyone knows, proved to be a reactionary. Now that we are given the proper political labels, our view of totalitarianism should become clearer. Stalin's atrocities should seem a little less unspeakable and his policy of genocide perhaps a bit more condonable. After all, murdering people in the name of reaction is not the same as killing them for the sake of social progress! The first must be violently condemned as an inhumanity, while the second may be indulgently viewed as an incidental *faux pas*. This is the sophism that Paterson and Adler suggest throughout their essay. No, they do not state it outright, for even they may recoil from the implications. But it is in the article if one only cares to stop and think.

Case Western Reserve University

ALBERT PARRY and PAUL GOTTFRIED

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

In their article on "Red Fascism," Les K. Adler and Thomas G. Paterson are very good when they stick to the topic, but their contentions become questionable when they begin to range wider afield. They are reasonable and believable when they document how the fascist and communist images began to merge in a certain segment of America's opinion, but they seem to lose their grasp when they begin to arrogate to themselves an understanding of fascism and communism. The footnotes in the article do not reveal an acquaintance with the literature on those subjects.

The authors purport to write from the viewpoint of higher rationalism, suggesting that America fell victim to a pernicious cold-war mythology. To explode myths is a worthwhile enterprise, but only when one does not become a victim of countermyths. The rationalism of the authors breaks down on at least the following three grounds. The contention that all commentators who saw a similarity between fascism and communism did not know what they were talking about depends on how one zeroes in on the problem, granting that there are differences between fascism and communism. For example, it appears to be completely rational to say that Nazi Germany and the Soviet Union are more similar to each other than either is to Great Britain or the United States. The assumption that first-hand experience is biased and undependable and that abstract study is of a higher quality strikes me more like escapism and unwillingness to face evidence than rationalism. My experience with historical evidence has taught me that, although some contemporary observers lie and some may make mistaken conclusions, the historian does not have a higher source of wisdom from which to form his conclusions than does a contemporary, and he may fall into the same errors as do the

contemporaries. The assumption that seeing similarities between fascism and communism is a uniquely American phenomenon is false. Perusal of protest literature from the Soviet Union will reveal the existence of a similar pattern of thought, and it must be remembered that George Orwell's *1984* is a work not easily accessible to Soviet dissidents. Ethnocentrism is not new to history, but it is generally assumed that it is undesirable for historians. In this article we encounter, of course, a very perverse case of ethnocentrism, but that should not make a difference. The authors are attributing an unusually high degree of "stupidity" to Americans and their opinion makers. It seems highly futile for a historian to refute the "stupidity" of Americans when there are still thousands of Eastern Europeans to contend with. Universality of truth may be unobtainable, but that does not mean that a historian should not venture outside the frontier of his country—especially when the discussion is about internationally used concepts.

The major failing of the article is its refusal to discuss the subject that it purports to discuss. It does not talk about totalitarianism as much as it talks about the mistaken judgments that Americans have made about the Soviet Union. If one wants to argue against the cold war it is perfectly respectable to do so, but one should not be surreptitious about it, and one should not do it at the expense of historical evidence. For example, the following sentence is typical for its implied judgments and invidious insinuations: "Thus on the eve of World War II, many Americans linked fascist and Communist ideologies as denials of human freedom and tolerance, saw Germany and Russia as international aggressors, and pictured Hitler and Stalin as evil comrades."

It is difficult to make out what the authors wanted to communicate in this sentence. Did they want to say that the Soviet Union was not an aggressor, or that Stalin was not a comrade of Hitler's? One can agree that, even in view of the Molotov-Ribbentrop Pact, there may be serious questions about the second meaning, but the authors also want to suggest that the Soviet attitude toward, let us say, the sovereignty of Poland was different than that of the Nazis. After raising this and other interesting propositions, the authors simply fail to show in what this difference consists.

Even more misleading is the following sentence: "The German experience, however, seems to have stamped the image of the concentration camp, with all its overtones of mass extermination and unbridled terror, on the Russian camps." It is never made clear whether the authors mean to say that there were no camps in the Soviet Union or that there was no extermination of people. In either case Khrushchev's de-Stalinization speech would contradict the inferred conclusions of the authors.

No single label ever describes any government very accurately—and I agree with Adler and Paterson that the label "totalitarianism" does not describe the realities of the Soviet Union very accurately—but I do not think that they proved their case. The liquidation of the cold war, as all of us know, is a noble goal, but the nobility of the goal is not a substitute for argument, knowledge of existing literature, and examination of evidence. The word "totalitarianism" implies a much broader scheme and type of control and mobilization of a country's resources than the authors seemed to be aware. The aspects of aggression, terror, and extermination of peoples are only parts of the concept. The authors were very good in showing how the concept of totalitarianism in its applicability to the Soviet Union emerged in the United States, but they should not have ventured to refute the validity of this applicability without having become acquainted with the many-sided problems whose discussion should precede such a refutation.

Ithaca College

ANDREW EZERGAILIS

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

On page 1050 of the April 1970 *AHR*, in the article "Red Fascism," by Les K. Adler and Thomas G. Paterson, there is mention of "The liberal Presbyterian columnist, the Reverend Abraham J. Muste." I believe the person meant is the late Arthur J. Muste, who was a Presbyterian minister, a liberal, and a well-known pacifist. If this is true, I think the error is serious enough to deserve a correction notice.

University of Bridgeport

RALPH H. PICKETT

TO THE EDITOR OF THE AMERICAN HISTORICAL REVIEW:

I wonder what was the purpose of publishing "Red Fascism" by Les K. Adler and Thomas G. Paterson in the April 1970 issue. The thesis of this article is that "totalitarianism," as a concept embracing both Nazi Germany and Soviet Russia, emerged at a time when the United States was in conflict with these two powers. This is hardly news to most of us; with a slight additional effort, the authors could have unearthed much more evidence to this effect.

The implied conclusion of the article, namely that because the concept of totalitarianism arose in the heat of political conflict it has no validity, is important and to some extent novel, but it requires proof. Anyone who makes this point must show that the differences between Nazi Germany and Soviet Russia are greater and more significant than the similarities and that therefore it is misleading to subsume the two systems under one term. Messrs. Adler and Paterson avoid this task and offer us merely the judgment that "because the outward appearances of the two systems seemed to be more similar to each other than either seemed to be to any previous political system in the world, the real differences between fascist and communist systems have been obscured." They fail to indicate what these "real differences" are, preferring instead to investigate the political motives of those who coined and used the concept of totalitarianism.

The authors do not quite deny the existence of the Soviet-Nazi Pact of August 1939, but by dismissing the standard State Department edition of captured Nazi documents on this pact as "the work of propagandists and not of scholars" (with an undocumented attribution to Walter Lippmann), they almost imply that there is something sinister about attaching importance to this event. The documents in question are authentic beyond doubt, and their authenticity is not vitiated by the fact that the State Department had political motives in mind when it made them public.

Questioning of motives is no substitute for evidence, and the cause of scholarship is not advanced by such procedures as those employed by Messrs. Adler and Paterson.

Center for Advanced Study in the Behavioral Sciences

RICHARD PIPES

THE AUTHORS REPLY:

For the most part these critical letters demonstrate a truism to us: readers often read into an essay what they want to find rather than deal with what is actually there. We admit to having approached a complex and obviously still sensitive historical problem, but the misrepresentations and misreadings of our article convince us that our critics have been stimulated by more than our ambitious title and project and those difficult questions that arise in any intellectual inquiry. It troubles us greatly that some readers facilely assumed that because we offer historical criticism of American foreign policy and thought we are thereby apologizing for Soviet foreign policy and Stalin's gory record.

It is long past time for scholars to halt this unproductive name-calling and to get to the task of discussing through the use of evidence the complexities of the cold war, the mutual responsibility for its origins, and America's significant role in the diplomacy of the recent past.

Clearly, as we stated, there were similarities between Nazi Germany and Soviet Russia; but clearly also there were some differences. For example, we reported that some contemporaries like George F. Kennan, as well as some scholars like Adam Ulam, did not believe that Stalinist Russia intended a military sweep over Western Europe. This is no minor distinction, and it is crucial in cold-war diplomacy and in our understanding of it. What our critics seem to have missed in their often emotional responses to the article's subject matter was the thesis itself and those qualifying statements that were meant to direct the focus of the study on the problem at hand. For the sake of clarification, let us quote a main point: "Indeed, as distinguished scholars have written in the postwar period, totalitarian systems have exhibited undeniable similarities. Yet it did not follow that Russia and Stalin in the cold war would always act in a manner similar to Germany and Hitler or that Russia was set inexorably on the path of military aggression" (pp. 1062-63). Or again: "What is more important for this discussion, however, is not that they were different, but that many Americans took the unhistorical and illogical view that Russia in the 1940's would behave as Germany had in the previous decade" (p. 1061). These statements are the crux of the essay, and are repeated in varying forms on pages 1047 and 1060. The first page of the article reads: "This popular analogy was a potent and pervasive notion that significantly shaped American perception of world events in the cold war" (p. 1046). We still maintain that the analogy has misled diplomats, blinded Americans, obstructed diplomacy, produced misconceptions about Russian militarism and revolution, heated up cold-war rhetoric and military budgets, and generally made agreements between Russia and the United States difficult. None of the critics confront this major point, but instead draw implications, make inferences, and contribute distortions that detract from our argument and miss its thrust.

We do not think that we should be so heavily taken to task for not having written other essays the critics would like us to have written. It was not our intention to write individual histories of the Greek Civil War, Stalin's ghastly record, the Soviet intrusions into Eastern Europe, especially after 1947, the Nazi-Soviet Pact of 1939, or Soviet thought about the analogy. They, of course, deserve considerable attention; but we wrote an essay on "Red Fascism" in American thought and would never presume to undertake the task of writing about all of these other questions in the few pages allotted to us in the *AHR*. We will give, and have given, some of these subjects treatment in our other work, as have numerous other scholars. It is unfair to us, for example, to isolate one sentence on the Greek Civil War from our essay and to belabor our inattention to the question. We agree that generalizations are a necessary nuisance, and we will attempt to reply below in more detail to the controversy over the turmoil in Greece.

Mr. Rudgers accuses us of several shortcomings. Let us reply in order. We indeed do make an effort to measure the impact of the rhetoric on public opinion; our evidence is substantial, drawing upon popular magazines, newspapers, radio broadcasts, and polls. And we point out, too, through research in published and private sources, that top-level government officials such as Harry S. Truman, J. Howard McGrath, General John Deane, Tom Clark, Willard Thorp, Arthur Vandenberg, and George V. Allen shared the popular analogy and stated it constantly to reinforce it in the public mind. And these are men who made critical foreign policy decisions. We do not deny that

men like Acheson, Truman, Kennan, and Harriman were "men of intelligence"; we do not agree, however, that it necessarily follows that their decisions were informed or correct, or that they avoided stereotypes. We recommend the "Truman Doctrine" speech of March 12, 1947, and the eight volumes of Truman's *Public Papers* for numerous simplisms uttered by the president and for distorted stereotypes both of his critics and of the Russians. We cannot ignore Truman's criticism in September 1946 of Henry A. Wallace: "The Reds, phonies and 'parlor pinks' seem to be banded together and are becoming a national danger. I am afraid they are a sabotage front for Uncle Joe Stalin." (From William Hillman, *Mr. President: The First Publication from the Personal Diaries, Private Letters, Papers, and Revealing Interviews of Harry S. Truman* [New York, 1952], 128). Or let us quote Clark Clifford's advice to the president in 1947 on how to defeat Wallace in the 1948 election: "Every effort must be made *now* jointly and at one and the same time—although of course, by different groups—to dissuade him and also to identify him and isolate him in the public mind with the Communists." ("Memorandum for the President," Nov. 19, 1947, Clifford Papers, Harry S. Truman Library, Independence, Mo.) And importantly, is intelligence the critical question? Perhaps as significant are the values and stereotypes—the ideology—of American leaders, for it is through the ideological screen that the facts, the cold-war data, are perceived. There have been a good number of intelligent men in high posts; but they have not possessed a monopoly on judgment, success, or infallibility.

Mr. Rudgers chose the Greek civil war, rather than our thesis, to discuss, and we are pleased to have the chance here to elaborate on our few comments in the article. The monarchy was supported and largely controlled by the British until domestic difficulties forced Britain in February 1947 to appeal for American aid and intervention in order to avert the collapse of the regime they dominated. There were a variety of politicians and people behind the British-Greek government, but the monarchists, the rightists, were most prominent, especially after their victory in the elections of March 31, 1946, which insured the return of the king. It is distorting to imply that the rebels were simply Communists; Communist-led they certainly were. But among the rebels were large numbers of peasants, political leftists, and people disgusted by the repression and corruption of the Athens regime. The army was filled with monarchists, as well as some former officers of the Nazi security forces in Greece. Non-Communist professors and civil servants were indiscriminately dismissed and jailed, the Greek Federation of Labor was purged, and even the Liberals were alarmed by the repressive character of the Tsaldaris regime in 1946. There were good and Greek reasons, then, to oppose the British-backed government. It must be pointed out that in 1944–45 George Papandreou was a British puppet whose own government was responsible for killing Greek leftists. O'Ballance points out that Papandreou offered to resign in late 1944, but the British would not allow it. We would suggest, then, that the direct British manipulation of governments and use of troops against dissidents after the cease-fire in February 1945, when the EAM controlled most of Greece, was not "in order." It is true that Albania, Yugoslavia, and Bulgaria assisted the Greek rebels, largely through use of their territory for sanctuary. But Mr. Rudgers ignores both the complexity of the question and too much of the history. There was a vigorous fight within the Greek Communist party between pro-Stalin and pro-Tito factions. The Stalinist group ultimately won out, and in retaliation Tito closed his territory to the Greek guerillas. To get any aid of importance from their Communist neighbors, the Greek rebels had to agree to the desires of those neighbors for chunks of Greek territory. In short, nationalism was at work here, and the Tito-Stalin schism worked against effective outside assistance. Russia paid little attention to the Greek Communists, lectured Tito on the

necessity of avoiding an international flare-up over the issue, recognized the British-backed regime, and seems to have held to the October 1944 bargain struck between Churchill and Stalin that Greece was in the British sphere of influence. O'Ballance points out the "tenuous" connection between the Greek and Russian Communists. Mr. Rudgers ignores the American role in this Greek conflict and never asks whether it was in the American national interest (as Walter Lippmann asked in 1947) to become ensnarled in the affair. There is, too, quite a difference between propaganda (Soviet) and millions of dollars, military advisers, and intervention (American). It is not "poor scholarship" to quote Richard Barnet's *Intervention and Revolution*, which we think is a perceptive study, because he has summarized and synthesized much of the literature on the Greek problem, including Xydis, O'Ballance, and Kousoulas. We would add to Mr. Rudgers' list the works of Stavrianos, Rousseas, McNeill, and Djilas, and would plead that a scholar's arriving at a different interpretation from the "traditional" one does not *ipso facto* mean poor scholarship. Finally, our point still holds: the Red Fascism analogy obscured American understanding of the complexities of the Greek Civil War, and hence helped lead to the simplistic Truman Doctrine. To quote Barnet again: "The fifth-column analogy from World War II dominated official thinking. The possibility that men had taken to the hills for reasons of their own and not as agents of a foreign power was never seriously considered" (p. 121).

As to Mr. Rudgers' third paragraph, we did not write that Soviet security was necessarily served by the imposition of Communist regimes. What we did say, although perhaps not as directly as we do here, was that American foreign policy refused to admit that Russia might have security fears and its own definition of its national interest. The fears may have been exaggerated, but they were fears nevertheless, and American diplomats, rather than reducing them, exacerbated them through the use of overt diplomatic pressure in Eastern Europe. One cannot ignore the statements of Russian leaders, so often repeated, that their country had been invaded twice within one half-century by Germans who marched through a weak, anti-Soviet Poland. To understand this intense Russian sentiment is not to condone what Russia did in Eastern Europe, especially after 1947. Rather, it is to point to a *fait accompli* that America had to deal with and could not change through its atomic, military, or economic power. The question was, then: How could the United States make the best out of a bad arrangement of power in order to preserve the independence of Eastern European states? American policy stubbornly and clumsily refused to accept the *fait accompli* of Soviet influence in Eastern Europe and loudly, through vociferous rhetoric, atomic diplomacy, nonrecognition, and the diplomatic use of economic power (trade and loans) directly challenged the Soviet Union, arousing those fears and helping to pull down the iron curtain from the American side. Mr. Rudgers cannot ignore the chronology that indicates a mixed Soviet behavior pattern before 1947. Indeed (see our note 86), he is applying a post-1947 behavior pattern on the 1945-46 period and thereby distorting history. The mixed pattern is demonstrated, for example, by independence and democratic politics for Finland and Czechoslovakia (until 1948); free elections in Hungary in the fall of 1945, with the Communists polling less than twenty per cent of the vote; a firm hand in Poland and Rumania; elections in Austria in 1945; and lack of success in controlling Yugoslavia and Tito. It is not until 1947 that Russia begins to tighten its grasp over Eastern Europe, to form a political and economic bloc, the Cominform. The important question must be confronted: What role did American policy play in causing that Soviet tightening? Kennan and others have pointed out, for example, that the Czech coup of February 1948 was in large part a Soviet response to the anti-Soviet Marshall Plan. Rather than assisting Czechoslovakia with valuable aid before 1948, the United States

cut it off in the fall of 1946, defining that precarious nation as simply a member of a Soviet bloc. The tortured lamentations of non-Communist leaders Jan Masaryk and Hubert Ripka over the failure of the United States to offer economic help and the detrimental effects of American policy are clear in Trygve Lie's *In the Cause of Peace: Seven Years with the United Nations* (New York, 1954), 233-34, and Ripka's *Czechoslovakia Enslaved* (London, 1950), 49, 311. In other words, American policy saw blacks and whites but no greys, and hence failed to see the differences between independent governments and satellite governments who were both friendly to Russia. In short, it is questionable whether American foreign policy, except in the cases of Finland (to which we gave aid before 1948) and Yugoslavia (to which aid was given after the break with Stalin), contributed at all to the preservation of the independence—mere scraps in some cases—of Eastern European countries against Soviet imperialism. Let us also quote Norman A. Graebner here: "Why did the United States after 1939 permit the conquest of eastern Europe by Nazi forces, presumably forever, with scarcely a stir, but refused after 1944 to acknowledge any primary Russian interest or right of hegemony in the same region on the heels of a closely-won Russian victory against the German invaders?" (Norman A. Graebner, "Cold War Origins and the Continuing Debate: A Review of Recent Literature," *Journal of Conflict Resolution*, XIII [1969], 131.)

We do not reject out of hand the importance of ideology in Soviet foreign policy; clearly it is a part of Soviet behavior. But we believe it mistaken to view Soviet policy solely through an ideological lens. This ideology is important, of course, only since the Bolshevik Revolution. (Why choose the Napoleonic Wars for an analogy?) But if Mr. Rudgers is going to make a case for the predominance of ideology, he will have to explain to us the business deals in the 1920's between American entrepreneurs such as Henry Ford and Averell Harriman and the Bolshevik regime, the Nazi-Soviet Pact, the alliance between capitalist America and Communist Russia during the war, and Russia's recognition of Chiang in China, among other examples.

We regret sincerely with Mr. Rudgers that there were cruelties and horrors committed by the Hitlerite and Stalinist governments. But it still does not follow that they would act alike in foreign policy, or that Russia would replay Germany's role in the 1940's. It is at this point also that we must reply to the often sarcastic and intemperate remarks of Messrs. Parry and Gottfried, remarks that hardly serve the cause of intellectual inquiry. We assumed that, at least in the community of scholars, it was recognized that to attempt a critical examination of American foreign policy in the cold-war years implied neither that one was a Stalinist nor a Soviet apologist. In what we believe to be a thorough and responsible study, we did not condone or excuse Soviet or Nazi crimes; we deny also the additional charge that we simply belabored American leaders for having committed an "inexcusable blunder." If there was a basic conception underlying our arguments—as several of our critics imply—it was simply that American understanding of certain world events was filtered through a perceptual glaze that highlighted a few aspects of a complex reality at the expense of others, with the result that national leaders were acting within the confines of an analogy that in important respects was not an accurate depiction of that reality.

Many of the points above apply to Mr. Ezergailis' communication. A rereading of our essay, we think, will reveal that Mr. Ezergailis unfortunately missed a good number of points and much of our discussion and evidence. His second paragraph is clouded and disjointed, and we are not certain of its meaning. We sense a deep distrust of historians and historical inquiry and recoil from the thought that we possess or practice "higher rationalism," "escapism," or "ethnocentrism," or have access to a "higher

source of wisdom" or "universality of truth." Such terms merely confuse the issue. The point is that we and he simply have different interpretations of the past. We do not grant that we have committed a "very perverse case of ethnocentrism." We did not undertake a study of British or Soviet thought; we studied as best we could the phenomenon of Red Fascism in American thought. But to shed the charge anyway, let us state that we have found statements by Soviet leaders that make an analogy between Nazi Germany and capitalist America (and Britain), and they are equally distorting and harmful to both truth and Soviet-American relations. (See Stalin's interview with *Pravda* after Churchill's "iron curtain" speech, in *The Origins of the Cold War*, ed. Thomas G. Paterson [Lexington, Mass., 1970], 5-8.) Mr. Ezergailis' list of "invidious insinuations" allegedly part of our article is both simplistic and abusive. Nowhere in our writing do we deny the fact of Soviet aggression against Poland in 1939 or the existence of forced labor camps in Russia either before 1917 or after. We do suggest that the term "totalitarianism" and the image of "Red Fascism" are far too narrow and loaded to explain either these historical facts or other vastly complex events of the 1930's and 1940's. And yet, as we amply demonstrate, it has been this mental model that has dominated much public and academic thought during and since that time.

Mr. Pickett's letter sent us to check our source and the initials of A. J. Muste. Our article was correct in giving his name as "Abraham." He was a leading pacifist and occasional Presbyterian minister. Although he wrote numerous essays and columns, we were not quite precise in calling him a "columnist." (See Nat Hentoff, *Peace Agitator: The Story of A. J. Muste* [New York, 1963].)

Finally, let us comment on Mr. Pipes' letter. He found a thesis we did not argue. Indeed, we agree with him that had such a thesis been argued, it would hardly have been worth the writing of an article. Our thesis was clearly not that the concept of totalitarianism emerged at a time when the United States was in conflict with Germany and Russia. Our discussion—consisting of one page early in the essay—developed the use of the term in America, and we would certainly not claim to have "unearthed" any "news" by reporting that development. Indeed, we relied upon the works of Spiro, Diggins, and Deutsch, and others, and we thank them for having unearthed material for us. It is therefore very surprising to us that Mr. Pipes found the thesis he did. This misreading also makes his second paragraph faulty, and we again refer him to our statements on pages 1061 and 1062-63. Of course, we do not deny the existence of the Nazi-Soviet Pact; we, in fact, devote pages 1049-50 to it and state how "profoundly disturbing" it was to Americans. Our discussion of the publication of the Nazi-Soviet Pact documents also seems to have been misunderstood by Mr. Pipes. The authenticity of the State Department edition of the documents is not in question, and we ourselves passed no judgment on those scholars who were involved in its publication. We indicated the significance of the documents to the popular analogy, a point Mr. Pipes does not question, and suggested that their publication—separate from earlier German and Soviet documents—confirmed in many American minds the idea of Red Fascism and thus contributed to the exacerbation of the cold war.

In conclusion, let us add that we are pleased to see the *AHR* devote its pages to this dialogue. We urge readers of this exchange to read or reread our article and to pass their comments on to us. Like all historians, we are in a constant state of revision, and hence welcome helpful suggestions.

Sonoma State College
University of Connecticut

LES K. ADLER
THOMAS G. PATERSON

* * * * *

Recent Deaths

* * * * *

CHARLES C. ADLER, professor of history at Hamilton College, died August 24, 1970, in Moscow, where he was completing a book on late nineteenth-century Russia. Before he joined the Hamilton faculty in 1959, Dr. Adler taught at Harvard University, where he received the Ph.D. in 1958. He had also been a reporter on the Watertown, New York, *Daily Times* and had served in the army during World War II and in Korea. Dr. Adler was forty-seven.

STUART L. BERNATH, who taught American diplomatic history at California State College at Long Beach, died from cancer on July 3, 1970. Dr. Bernath was educated at the University of California at Santa Barbara and at Humboldt State College. A student of Anglo-American diplomacy, he wrote *Squall Across the Atlantic: American Civil War Prize Cases and Diplomacy* (1970). He also wrote on Civil War diplomacy for the *Journal of Southern History* and *Civil War History* and contributed several articles to the 1969 edition of the *Encyclopedia Americana*. At the time of his death Dr. Bernath had begun a study of William Randolph Hearst and American foreign relations. He was a member of the AHA, the Organization of American Historians, and the Civil War Roundtable.

WILLIAM CAMPBELL BINKLEY died in New Orleans, August 19, 1970, at the age of eighty-one. Professor emeritus of history at Tulane University, he had a distinguished career both in teaching and in the promotion of historical scholarship. Born in Tennessee, he received his Ph.D. from the University of California in 1920 and taught principally at Colorado College, Vanderbilt, and Tulane. To some he was best known for his teaching—particularly the twenty or more years at Vanderbilt, where he left the print of his dedication and scholarship on several generations of students. Editor of the *Official Correspondence of the Texan Revolution* (published in 1936 under the auspices of the Beveridge Memorial Fund) and author of several works including *The Texas Revolution* (1952), his scholarly interests extended more broadly to encompass the frontier and Southern history.

Although his writings have served as models for research and writing, his greatest contribution was in the promotion of historical scholarship. A leader of the Southern Historical Association during its formative years in the 1930's, he served as president of the association and as managing editor of the *Journal of Southern History*. In the 1940's and 1950's he was a leading figure in the expansion of the Mississippi Valley Historical Association, serving as its president and, from 1953 through 1963, as managing editor of its journal, now the *Journal of American History*.

Whether in his work with historical associations or as editor, he displayed a quiet modesty and a continuing sense of humor. At bottom, however, was a fund of knowledge that made some think he never forgot anything. Those who worked with him in association activities are familiar with his sense of dedication and his organizational insight. Those who worked with him on an editorial enterprise know that as a craftsman he was the envy of other editors. The time he spent on some manuscripts was incredible: he might take only a few minutes on a page but spend an hour on a sentence. He

cultivated not only known talents but opened the pages of the journals he edited to any contributor who could meet his standards.

His was a full life, the opposite of what he once wrote about two Texas heroes, William B. Travis and James W. Fannin, that they are "remembered because of the way they died rather than for what they accomplished while living." Professor Binkley will be remembered for his accomplishments, and especially for the help and encouragement he gave to others. A large part of his pleasure came from those out-of-office occasions on which he and his wife did so much to warm the lives of students, colleagues, and friends. On this point testimonials abound throughout the South and West. The Binkleys were a remarkable couple, and Professor Binkley's wife, who survives him, can take comfort in knowing that they made the world they touched a better and a happier one.

Trinity University

PHILIP F. DETWEILER

HERBERT M. MORAIS, former professor of history at Brooklyn College, died March 13, 1970, at Laguna Hills, California. Dr. Morais was sixty-four. His publications included *Deism in Eighteenth Century America* (1934), *The Struggle for American Freedom, The First 200 Years* (1944), and *The History of the Negro in Medicine* (1967).

OSCAR OSBURN WINTHER died in Bloomington, Indiana, on May 22, 1970, after a long and courageous battle with the illness that claimed his life. Survivors include his wife, Mary, with whom he shared a deep love of humanity and respect for the human intellect; his daughter, Mrs. James R. Scobie, whose attainment of a Ph.D. degree in history brightened his last days; and his son, Eric, whose decision to carry on graduate work in history brought to poignant fruition one of his cherished hopes.

The son of Danish immigrants, Winther was born in 1903 at Weeping Water, Nebraska, where he spent his first nine years. The family then moved to the Willamette Valley in Oregon. There, at the Eugene High School and at the University of Oregon, his interest in history led him toward an academic career. After a brief stint as a high school teacher, he began his graduate education at Harvard. Receiving his M.A. in 1928, he returned to high school teaching to earn the capital necessary for further graduate work. In the fall of 1929 he entered the Stanford University graduate school, where he became a member of Professor Edgar Eugene Robinson's seminar. Under Robinson's direction he wrote his dissertation on the express and stagecoach business in California, a study ultimately published by the Stanford University Press.

After receiving his Ph.D. degree in 1934, Winther accepted a position as assistant curator of the Wells Fargo Bank and Union Trust Company. He also taught at Stanford and at the San Jose Adult Education Center before going to Indiana University in 1937. At Indiana he cheerfully assumed various duties in the department of history and in the university, duties that were often demanding and sometimes tedious. Appointed assistant dean of the graduate school in 1949, he became associate dean in 1953 and held that post until his resignation in 1958. Despite heavy administrative responsibilities, he continued to teach classes and pursue his own scholarly activities. During the last five years of his life he was university professor at Indiana.

Beginning with publication of his *Express and Stagecoach Days in California* in 1936, Winther produced an impressive array of books and articles. His early interest in transportation never flagged. Adding color and anecdotes to his account, he directed

his *Via Western Express and Stagecoach* (1945) to a more general readership than he reached with his first book. The development of transportation was an important theme in his general works, *The Great Northwest* (1947) and *The Old Oregon Country* (1950). And finally, he synthesized much of his previous research in his engaging study, *The Transportation Frontier* (1964).

For all his investigation of transportation, Winther's scholarly production was neither exclusive nor parochial. He edited two volumes of manuscripts, *A Friend of the Mormons: The Private Papers and Diary of Thomas Leiper Kane* (1937) and *With Sherman to the Sea: The Civil War Letters, Diaries and Reminiscences of Theodore F. Upson* (1943). In 1955 he translated and co-edited a third volume, *Diary of a Dying Empire*, by Hans Peter Hanssen. His bibliography of periodical literature on the trans-Mississippi West, first published in 1942 and brought up to date in 1961 and 1970, has been an invaluable guide to students and researchers. His co-authorship of *The Story of Our Heritage* (1962), a text for junior high school, bespeaks his concern for the teaching of history at all levels as well as his ability to write clear, unencumbered prose.

Winther was constantly on the alert for means to facilitate and improve the study of history. As president of the Indiana History Teachers Association in 1944 and as an advisor to school systems, he exerted an immeasurable influence. An abbreviated listing of offices he held suggests the breadth of his professional activity. One of the founding fathers of the Western History Association, he became its president in 1963-64. He served a three-year term as managing editor of the *Journal of American History*. At the time of his death he was president of the Oral History Association; he was also a member of the standing committee of six of the Joint Committee on Bibliographical Services to History, working under the general direction of the executive secretary of the American Historical Association.

Winther was proud of his ancestry. Having spoken Danish with his parents and English with his brothers, he was bilingual, and on several occasions he visited the land of his fathers. Traveling widely in other parts of Europe as well, he developed a special fondness for England. In 1966 he was visiting distinguished professor at the University of Birmingham. Having few interests that did not ultimately find scholarly expression, he spent several years collecting materials on British influences in the American West. Shortly before his final illness he read a paper, "The English in Kansas, 1865-1890," at a University of Kansas conference.

A man of kindly humor and highly developed comic talents, Winther was serious about making commitments—precisely because he seriously believed that commitments were made to be kept. A man who took care to recognize realities, he inspired hope and confidence—precisely because his mind moved imperturbably toward what was realistically possible. A man of monumental patience in his dealings with others, he was quick to resent any injustice—precisely because he accepted the obligations imposed by his understanding of human dignity. A man who lived as he chose, he profoundly influenced his colleagues, his students, his friends—precisely because he so consistently demonstrated the profound wisdom of the choices he made.

University of Wisconsin

PAUL W. GLAD

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AMERICAN HISTORICAL REVIEW

Volume LXXV

Compiled by JOHN T. APPLEBY

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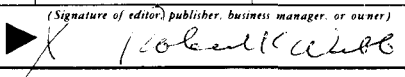
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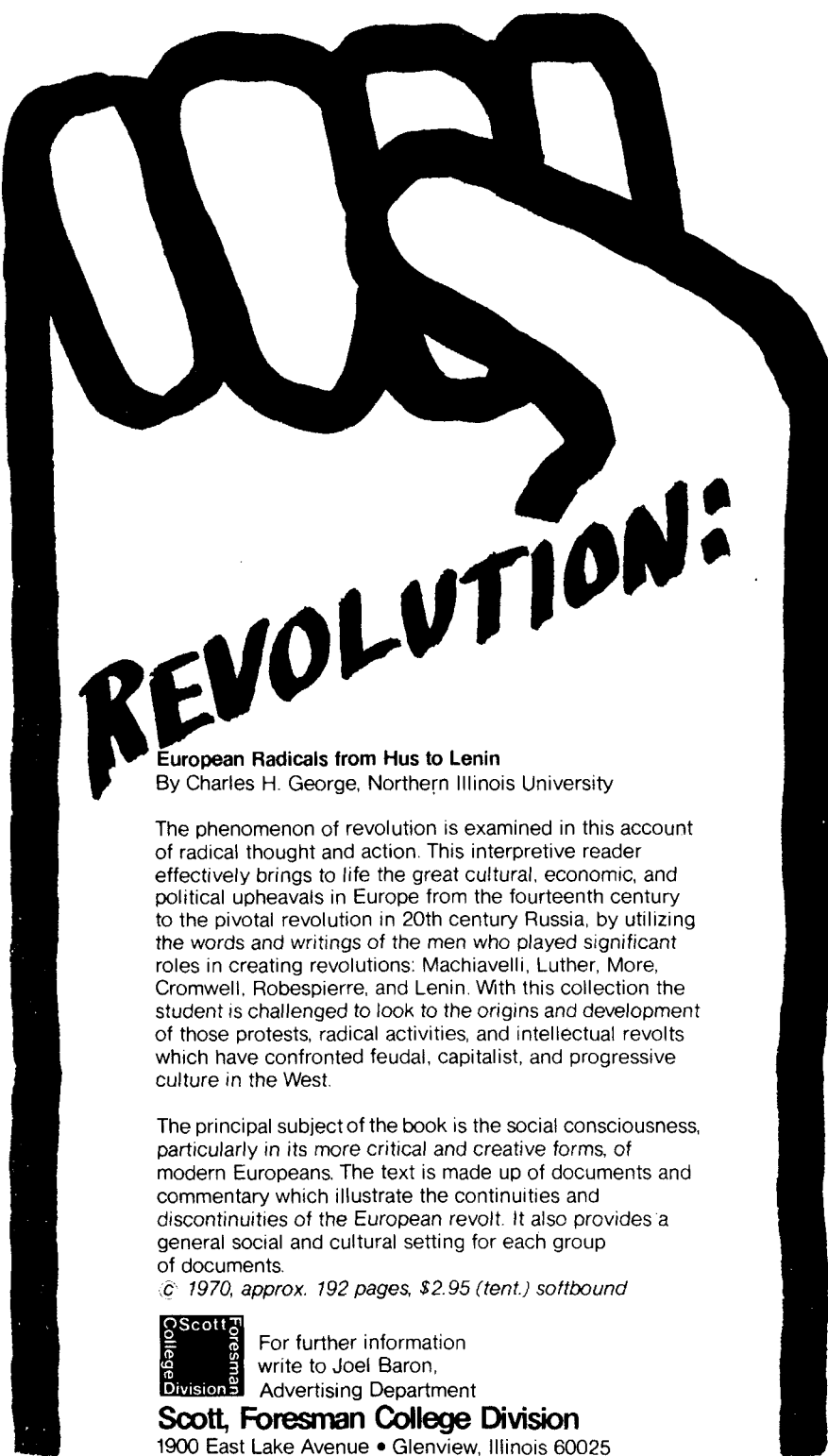
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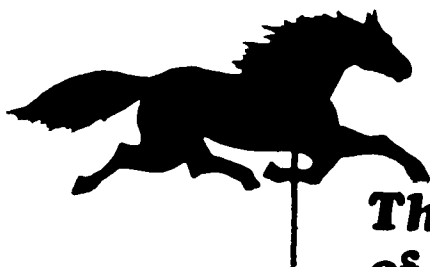
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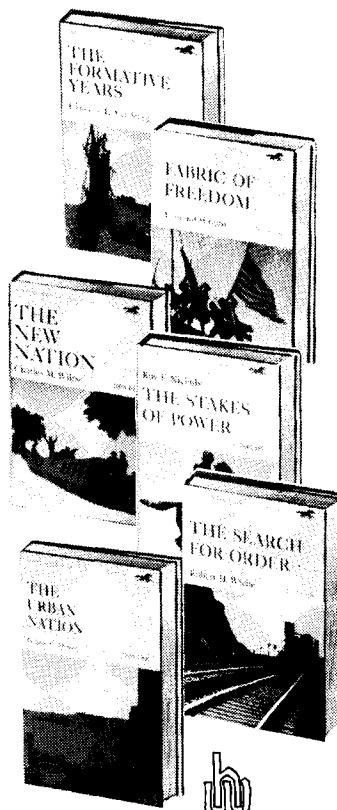
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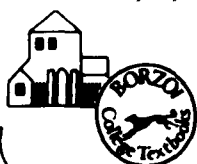
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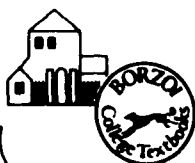
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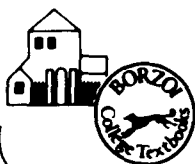
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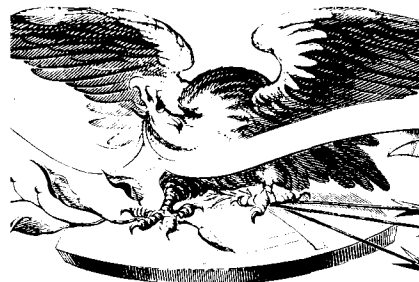
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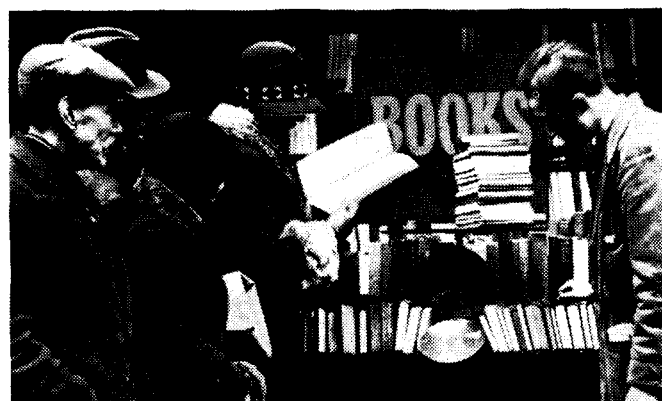


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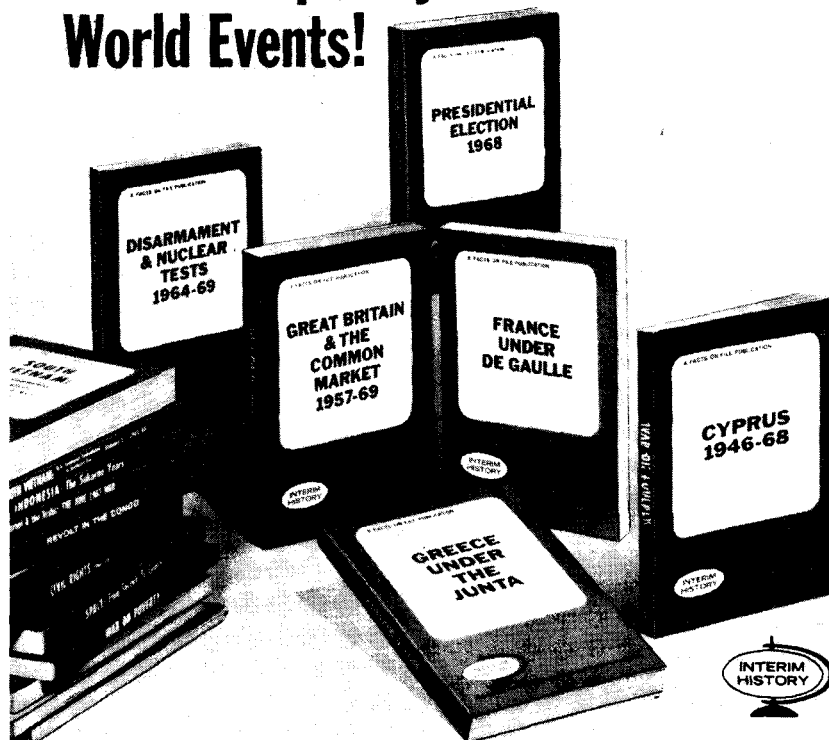
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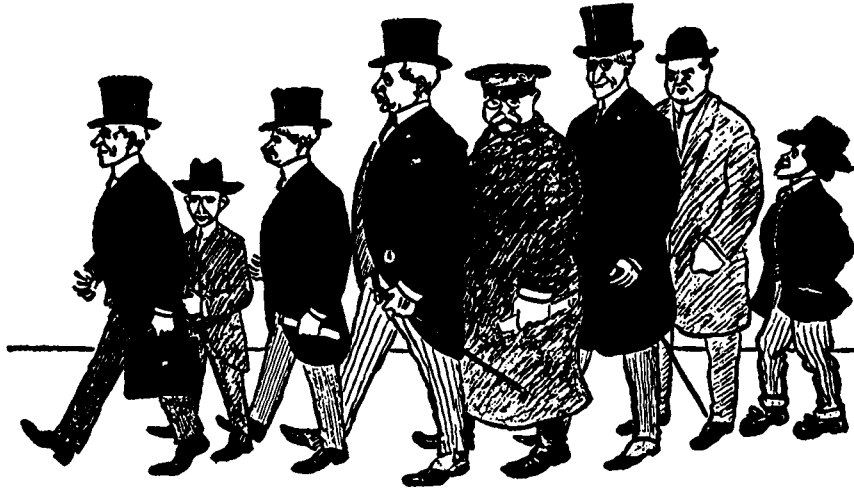
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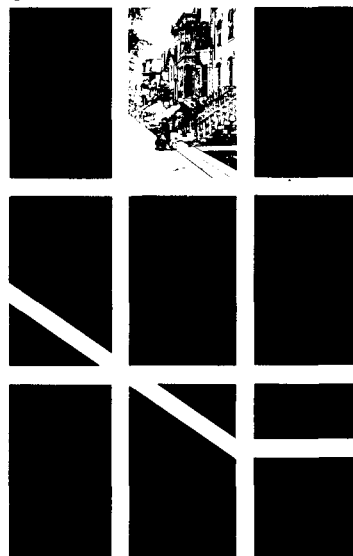
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